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Special Issue

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Special Issue

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MISSOURI BOTANI

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Although I could write at length about the many factors that have gone into this special issue, I'll confine my comments largely to one aspect—networking—to use a trendy concept. It is because of networking, I should imagine, that Steve Weber and Steve Emslie, conceived the idea of an issue of the *Journal* devoted to "new directions in ethnobiology", and no one knows better than Karen Adams and I that several of the papers in this issue would not have been submitted for inclusion if we had not had numerous suggestions and much interest from various ethnobiologists—some members of the Society, some not. It is a pleasure to acknowledge your splendid participation.

Society officers Weber and Emslie drafted the initial letter explaining their conception of the special issue and inviting scholars to participate through preparing a "state of the art" paper in their individual areas of ethnobiological involvement. When they submitted this draft, to be sent under my signature, they included a list of scholars they wished to invite. I added a number of names and sent out invitations. Some failed to respond, others offered encouragement but were overly committed to other projects and so declined, and a very few accepted. I next sent a letter to the 15 or so scholars on my list of "Tucson ethnobiologists", informing them of the special issue and requesting their help in assembling a cadre of authors. The response, although not overwhelming, was more than adequate to kindle the next stage.

One of the Tucson ethnobiologists, Joe Laferriere, suggested Stephen Brush and Ben Orlove from Davis, California and they, in turn, recommended Constance McCorkle, so it was through this piece of networking that we have a paper on ethnoveterinary medicine—an area of study about which I had been abysmally ignorant, my only previous contact with it having been a number of anecdotes of practices in Saudi Arabia related by my friend and colleague, Ted Downing. I trust you will find, as I have, this contribution on ethnoveterinary medicine as fascinating as it is unusual.

Here's an example of networking operating in a different way. Over the years Bob Bye and I have noted (as, most likely, have all of you) that from time to time, all ethnobiologists work in areas where they have had little training. We have discussed the need for ethnobiologists, not trained in biology, to know the value of voucher specimens. Bob wrote such a paper and although it had been planned for Volume 5, Number 2, unanticipated delays prevented its appearance, fortunately, until this issue—exactly where it should be published. Amadeo Rea and Harriet Kuhnlein were then inspired to write somewhat similar articles in their individual areas of ethnobiological study, so now we have three educationally oriented papers.

Through networking we found scholars with interesting ideas about "new directions in ethnobiology" who, at the time the invitation was issued, were not already so overly committed but that they could share these views with us. It will be interesting to see what networking will produce in the second special issue of the *Journal*, at some indefinite future time.

—W.V.

The Development of a Society:
An introduction to the special issue.

In partial response to increasing academic diversification, there has been a veritable proliferation of specialist journals available to scholars. While this trend has not always been universally welcomed, there are arguably some cases in which such a course of action is wholly justified. The foundation of the *Journal* and Society of Ethnobiology should be regarded as just such a case. My involvement with the *Journal* and Society goes back to its inception, and for the purposes of this special issue, which appears on the eve of the tenth anniversary of the first ethnobiology conference, it seems appropriate to consider anew some of the questions and issues surrounding ethnobiology which the institution of the *Journal* and the Society were partially intended to address.

After 10 years, it is still necessary to ask what is ethnobiology? Is it any more than a title or a cover term applied to various types of research which do not fit comfortably into other disciplines? Since ethnobiology has no unifying theory of its own, can it justifiably be considered as an interdisciplinary field, or would it be better to view it as a subfield of already long-established scientific realms? Finally, what common thread exists to link together the diverse researches which have appeared in this journal, or been discussed at the Ethnobiology Conferences? What is the rationale underlying the investment of time and energy in fostering the development of a conference, society and journal devoted to the integration of this research?

Ethnobiological research can be described as work that draws on both biology and anthropology to make statements about the interrelationship between living organisms and human culture, whether prehistoric, historic, or contemporary. Interest in this interrelationship is not new. Ever since anthropologists' initial emphasis on natural history, biology and anthropology have been intertwined. For example, there were the early attempts by anthropologists to classify societies on a scale of evolutionary development according to their mode of subsistence, where the appropriation of nature was regarded as a critical factor in determining the advancement of other aspects of culture. Later studies focused on the systematic relationship between a sociocultural entity and its environment, and stressed adaptation and change in ecological systems. More recently, interest in the development and spread of cultigens, domesticated animals and agricultural complexes, and their co-variation with social organization and population, have all helped to stimulate interest and research in the interrelationship between biology and anthropology.

There was a time when zoologists and botanists would be asked to contribute their expertise to the identification of biological materials derived from ethnographic and archaeological contexts without being expected to have any great insight into the research orientation as a whole. Nowadays, it is more usual for specialists to take an active role in model and hypothesis building that blends methods, concepts and models drawn from both anthropology and biology. Ethnobiological research is conducted within the constraints imposed upon it by elements of both biological and anthropological theory, and, in turn, its contributions are weighted according to the prevailing requirements and questions of these two super-disciplines.

The first Ethnobiology Conference was held in 1978 in Prescott, Arizona, and was sponsored by the Prescott Center College and organized by Steven D. Emslie. The second, in 1979, took place in Flagstaff, Arizona, and commemorated the contributions to the early development of ethnobiology of both Hargrave and Alfred F. Whiting. The proceedings of this conference were published in 1980 in the first *Journal of Ethnobiology*, under the auspices of the Center for Western Studies, a private corporation managed

by Steven Emslie and me. However, our intention in setting up the *Journal* was not simply to record the papers given at the Flagstaff conference, but also to provide a much needed forum in which articles of ethnobiological interest could be presented together. In the past, these had been scattered in specialist journals, archaeo-biological data appearing in archaeological journals, plant-human material in plant journals, and so forth, and we felt that this hampered their accessibility to all researchers in ethnobiology, and that the integration and fruitful development of the field could only take place once it was formally established in the framework of a journal.

The design we decided to use as the symbol of our newly created *Journal* was that of a split-twig figurine. The figurine, which is featured on all the covers of the *Journal*, neatly encapsulated its critical elements—for the original objects are pieces of plant material manipulated by humans to represent an animal. Split-twig figurines date to around 2000 b.c., and are found in the American Southwest. They are thought to have had some magical or religious significance for the Archaic hunter-gatherer peoples who made them.

When we chose to use the term 'ethnobiology' in the naming of the *Journal* (partly in deference to the title of the Conference), we were aware of the way in which the word breaks down into two elements, namely 'ethno'—(from the Greek 'etnos'—literally, race or peoples), referring to the human aspects of biological relationships, and 'biology', or the study of the entire range of living organisms. This seemed to render the term particularly appropriate for our purposes, since we wanted a title broad enough to cover the range of possible research orientation which relied on the integration of biology and anthropology. Perhaps this appears somewhat self-explanatory. However, 'ethnobiology' is not always defined as broadly, nor with the same general outlook as we intended here. One definition I came across recently stated that 'ethnobiology' was "a branch of the study of ethnology which relates to the distinctive physical and racial characteristics of specific ethnic groups or population isolates."

Initially, we decided to put out two issues a year, one devoted to conference papers, the other to independently submitted articles. We invited a number of scholars to serve on the board, who were known for their achievements and interest in ethnobiology, and whose diverse research objectives and accomplishments were essential if the journal was to reflect adequately and intelligently the rich variety of data and ideas which could be subsumed within ethnobiology. For the first few years, Steven Emslie and I alternated editorial and managerial responsibilities. Then in 1982, we were elected the first President and Secretary/Treasurer of the Society of Ethnobiology, which we had founded a short time before as a non-profit organization whose primary purpose was to oversee the publication of the *Journal* and the organization of the Conference. The editorial board then proceeded to find a new editor for the *Journal*, and selected Willard Van Asdall, who has done an excellent job and contributed considerably to the *Journal's* success up to the present.

Alongside the foundation and development of the Ethnobiology Conference, *Journal* and Society, there has been continued diversification and growth within the field they were designed to reflect. It is important, then, that the Society adapts to new trends, and remains sensitive to the changing needs of its membership. At the time of writing, the Society is considering a number of projects for the future, including a monograph series, converting the *Journal* from a bi-annual to a quarterly issue, and instituting prizes and awards for achievement and excellence in ethnobiological research.

The concept of the special issue was conceived by Steven Emslie and me as a periodic means to chart and evaluate the research foci of a fast developing interdisciplinary field. We decided that this would be best achieved by inviting prominent writers and workers who site themselves within the broad realm of ethnobiology to submit articles with the assessment of current and future achievements in mind. We hope this will be the first

of several such special issues which will assist in the refinement and enrichment of ethnobiology as a whole.

This special issue covers a number of important topics and issues of ongoing concern in ethnobiological research. These range from the appraisal of well-established fields in the discipline to the evaluation of new topics and models. For example, Bohrer assesses the achievements of ethnobotany, and offers some suggestions for its future. Delving into a specific category of ethnobotany, Holloway and Bryant present a thorough and critical review of the uses and applications of pollen analyses. In the field of zooarchaeology, Lyman picks up the theme of species lists and discusses their heuristic potential. Among the relatively new subdisciplines of ethnobiology, a lesser known one is ethnoentomology, and in a useful article, Posey traces its history and, at the same time, considers the value of ethnobiology as a generative source of new ideas.

Bye's discussion of the use and role of voucher specimens represents an important statement on the documentation of biological material so that it is of maximum utility both to the researcher and to later users of the data. Rea is similarly concerned with documentation and verification and he makes a number of recommendations for the more stringent and fruitful performance of archaeofaunal studies. Kuhnlein's paper brings us to another immensely important field within ethnobiology, nutritional studies. She too considers problems of method, with specific reference to the collection and chemical analysis of food samples.

Interest in subsistence and cultural ecology remains as keen as ever. Winterhalder's paper on foraging models among hunters and gatherers, and Brush's on change in farming systems through the perspective of the loss of genetic diversity, mark two of the possible directions in which subsistence studies can go. Orlove and Godoy's article on Andean highland patterns of crop and pasture management elaborates on familiar cultural ecological themes in demonstrating the complex interaction between agricultural systems and social organization.

Another area of longstanding concern in ethnobiology revolves around the labelling and classification of elements of the natural world. This has developed into an important research orientation including, at one extreme, the simple elicitation of folk plant and animal names and, at another, the exploration of the ways in which people organize and think about their surroundings. Articles by Elisabetsky and McCorkle deal with new and intriguing trends in the study of folk models and categorizations, but these are especially interesting because they represent prescriptive systems that organize both thought and action. Ellen's paper, on the other hand, provides a valuable critique of the logical and philosophical underpinnings of the process of constructing folk classifications.

The articles presented in this issue reflect only a small portion of the types of research now considered to be part of ethnobiology, as a glance at the list of contents of back issues of the *Journal* will confirm. Over the years, article have appeared on such diverse subjects as nutrition, domestication and subsistence, environmental reconstruction, folk classification, and questions of method and theory. While we initially intended the *Journal* to merely reflect the field of ethnobiology, and to encourage the kind of effective communication among scientists that we thought would be essential if ethnobiology were to become truly interdisciplinary, it has become apparent that through the selection, editing and publication processes, it has actually helped shape ethnobiology.

Ethnobiology is growing, and more scientists are prepared to identify their work as such. The simple addition of the prefix 'ethno' to a word associated with the natural sciences happens increasingly, and signals the inclusion of even more new and fruitful topics under the umbrella of ethnobiology. Perhaps this reflects a specific characteristic of scientific inquiry today, namely the division and recombination of elements from several fields. The richness and potential of blending anthropology and biology in this way will, I hope, continue to be domonstrated by ethnobiology. Already, as Bohrer tells

us, ethnobotany is beginning to capture public interest. No doubt this is true as well for other aspects of ethnobiology. Moreover, as ethnobiological objectives focus on broader issues of the integration of cultural and natural systems, as well as change, ethnobiology as a whole may be in a better position to develop theory of its own, as well as to make a richer contribution to anthropological theory than in the past.

The future of ethnobiology looks promising. Developments in techniques of data recovery, a growing body of linguistic, ethnographic, archaeological, historical and experimental data, and a better understanding of natural and cultural processes are, of course, critical. However, it is the continual creative integration of ideas and data brought into ethnobiology from many different sources that is most likely to ensure the continued growth and improvement of this newly interdisciplinary field.

Steven A. Weber
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VOUCHER SPECIMENS IN ETHNOBIOLOGICAL STUDIES AND PUBLICATIONS

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ABSTRACT.—Voucher specimens are a critical part of ethnobiological studies. They physically and permanently document data and form the basis for review or reassessment of the original study. An adequate voucher specimen must have diagnostic characters, be preserved in the condition, be accompanied by appropriate field data, and be maintained and readily accessible in a suitable repository institution. Planning *prior* to the collection of specimens is essential. Because they differ from taxonomic samples, special attention must be given to the collection and maintenance of ethnobiological specimens. In ethnobiological studies, the scientific name is based upon the identification of the voucher specimen and serves as the crucial link between folk knowledge and Western science.

INTRODUCTION

The voucher specimen is a critical component of ethnobiological studies. It provides the documentation for the scientific identity of the biological material about which observations and data are recorded. To date, voucher specimens have been accepted in some, but not all, disciplines of natural history. With the need to maximize the value and use of these collections, the Association of Systematics Collections has published a report[2] on voucher specimen management (Lee *et al.* 1982).

A voucher specimen is an organism or sample thereof "which physically and permanently documents data in an archival report by: (1) verifying the identity of the organism(s) used in the study; and (2) by doing so, ensures that a study which otherwise could not be repeated can be accurately reviewed or reassessed" (Lee *et al.* 1982:5). In order for a voucher specimen to fulfill its function, it must: "(1) Have recognized diagnostic characters that are appropriate to the level of identification in the report. Specific life stages or body parts may be required. (2) Be preserved in good condition by the investigator/collector according to acceptable practice. (3) Be thoroughly documented with field and/or other relevant reports. (4) Be maintained in good condition and be readily accessible in suitable repository institution" (Lee *et al.* 1982:7). The following points need to be emphasized so that a voucher specimen meets the requirements. First, the material preserved is a sample of the organism or the population that is actually studied. Second, the sample is adequate for identification and is deposited in an institution where it is cared for and made available to researchers. Third, essential data collected with the sample are physically associated with the specimen. Fourth, the archival report (published or unpublished) makes reference to the voucher specimen. Further details on the practice and justification of voucher specimens are found in Lee *et al.* (1982).

For the ethnobiologist, voucher specimens are essential to his/her work. The specimen is the basis for identifying the organism. Matching a common name with a scientific name is not identification (see Mead 1970). The identification to at least the species level provides western scientists a common basis for comparison of biological, ecological, and cultural data as well as for reevaluation of the information. Although occidental scientific identification, classification, and nomenclature have their limitations, they are more easily applied on a universal level than folk taxonomies.

With an accurate identification, the scientific name becomes the crucial link between people with folk knowledge and people trained in western sciences. Researchers can correlate and compare information generated and modified by generations of humans with that derived from more recent occidental scientific pursuits. The scientific name is the primary structure for bridging two cultures and for facilitating the mutually beneficial exchange of information. The return of knowledge to communities from which it originated is a critical component of ethnobiology today(Gomez-Pompa 1982; Toledo 1982) and can take many forms (e.g., native author publications, cultural rescue programs, health improvement projects, etc.).

The use and appreciation of voucher specimens have been greatest among biological taxonomists. Consequently, they have set the standards for the formation and management of these materials. As other disciplines such as ethnobiology, ecology, and environmental impact studies develop the role of voucher specimens expands—and so too do the potential uses and the problems of application.

The verification of identification and the change of the name (because of more accurate identification, up-dating nomenclature, or revised classification) are most efficiently carried out when the specimen is available to taxonomic specialists through normal channels such as revision of the holdings of recognized repositories or museum collections. Consequently the value of the specimen constantly increases for the taxonomist as well as for the ethnobiologist who can periodically consult his/her specimens for current identifications or annotations. Also, the specimen may yield more data such as chemical, ecological, and structural information for subsequent studies that were not part of the original investigation (e.g., Condon and Whalen 1983; McCain and Hennen 1986). Other disciplines may also benefit from the specimens as sources of data not otherwise tapped in their work. For example, biogeography can be aided by distributional data from collections of an ethnobiological study in an inaccessible area.

The early developmental and pre-reproductive forms of plants and animals are frequently encountered in ethnobiological studies and are not usually acceptable as specimens by taxonomists. However, the plants and animals may be in a non-reproductive state at the stage of recognition and employment by native people. Consequently, taxonomists are often reluctant (and sometimes refuse) to identify and manage such ethnobiological material. This frustratring problem can actually be resolved to the benefit of both the ethnobiologist and the taxonomist. Often structural characteristics, such as juvenile leaves of edible herbs, have been ignored by taxonomists and are not considered to be important or diagnostic. But these same characters may be critical to traditional people who rely upon the tender plants for food. Rather than considering this situation as an impasse to multidisciplinary cooperation, it provides fertile ground for contribution of new data to both areas of investigation. Looking at plant population studies of seedlings as an example, ecologists needed an easy system for identifying in a non-destructive manner germinating seeds and seedlings (Duke 1969). Such characters and life stages were not routinely considered by taxonomists. When the forms of germination and the types of seedlings demonstrated repeatable patterns, taxonomists began to incorporate these characters into their descriptions and identification keys. With ethnobiologists working with native people who are keenly aware of the characteristics of the various life cycle stages of plants and animals, sufficient stimulus should be

present to encourage our colleagues to critically examine underutilized chemical, structural, ecological, and life history features.

CATEGORIES OF VOUCHER SPECIMENS

A voucher specimen for organisms that are studied or observed may consist of one or more of the following categories (Lee *et al.* 1982:6-7): (1) the actual organism (whole or part); (2) a sample of one or more individuals (whole or part) from a population; (3) a representation of the organism or its characters (e.g., photographs, sound recordings, etc.), although it may not be adequate but may be the only alternative that is practical and/or legal; (4) an associated specimen that is biologically or functionally related (e.g., pollen preparations, fiber slides, stomach contents, pathogens, etc.) of the organism; and (5) a corroborative specimen that provides additional data or characters (e.g., from the same individual or population but at a different time or stage in the life cycle) to a previously collected voucher specimen.

For the ethnobiologist, the voucher specimen should be the organism actually studied or a sample that originated from the same population at the time of observation (categories 1 and 2). This specimen should reflect the characters, characteristics, and stage of life cycle about which the informant or collaborator provides information. Often the condition of the specimen may not reveal the diagnostic features (usually reproductive parts) that are required by the taxonomist for identification. Therefore, every effort should be made to *also* obtain a corroborative specimen that is collected from the same organism or population from which the original voucher specimen originated. This can be done by marking and recollecting later the organism or population in order to obtain material with reproductive and other taxonomically important features. In some cases the organism can be cultured, grown or raised until it reaches maturity, such as the case with a fresh root or seeds which can be planted and later pressed with flowers, fruits, and leaves for an herbarium specimen. In all cases, cross reference should be made between the original voucher specimen and the corroborative specimen and their relationship clearly noted. Sometimes, different collection numbers for the two specimens may be made but the connection between them should be specifically stated. Other times, the same collection number can be used for the two specimens but the different collection dates are noted as well as the relationship.

The other category of voucher specimens that is useful to the ethnobiologist is the associated specimen. Often the material under study (e.g., sample, phytoliths, wood fiber, seeds, exudates, bone tissue, etc.) is not the main character studied by the taxonomist but is the basic evidence used by the ethnobiologist. In such cases, a voucher specimen of the organism(s) is made according to the standard accepted by the experts studying that taxonomic group. Then, the special preparations of the parts (e.g., pollen, tissue sections, chemical extracts, etc.) are made from the voucher plant or animal specimen and analyzed. The results (products and data) are deposited along with a cross reference to the voucher specimen. Such special preparations may require separate management in ancillary collections such as those for pollen (palynological collection), wood (xylarium), fruits and seeds, stomach contents, etc. These associated specimens are linked directly to regular museum collections available to taxonomists. Also, the associated specimen should be duplicated so that one remains at the designated repository with the original voucher specimen and the other forms part of the investigator's comparative collection. Such collections with selective parts and data are then used to identify (by comparison and degree of similarity) unknown materials from archaeological or contemporary sources. These comparative collections should include the distinctive parts that would be encountered in pre-processed, processed, utilized, and disgarded conditions.

Today most collections of organisms are made so as to reflect the range of variation of characters of taxonomic and evolutionary significance. When an ethnobiologist produces voucher specimens, his/her collection should be a representative sample of the population. Also extra efforts should be made to obtain sufficient quantity of the parts required to make the associated specimens from each voucher specimen. Hence an adequate number of mice or herbs should be collected if associated collections of teeth or seeds are to reflect significantly the variation in these parts.

When using the comparative collection to identify unknown materials, the degree of similarity, the identity confidence, and the level of identification should be determined by establishing a basic set of quantitative and qualitative characteristics for each known taxon. Thus the identification can be determined objectively to a specific degree of accuracy. When reporting the new identification reference should be made to the characters used and the source of the comparative collection, including the citation of its voucher specimens. When reporting the identification of a previously unknown organism, one may not be 100% confident of the determination. The degree of confidence may be expressed by listing only the name of the taxonomic level at which one is sure. For example, if you are confident that the fruits are those of the member of a family, you may cite Chenopodiaceae. If you believe that they are members of a given genus, can report *Chenopodium* sp., or the subgenus if possible (e.g., *Chenopodium*, subgen. *Chenopodium)*. Should you be confident that the material is of the species, you can provide the name with the generic name, specific epithet and the author (e.g., *Chenopodium album* L.).

But what if you are not sure about this identification? If the original material is in good condition and is more similar to that species than others in the area (but the other species may have indistinguishable fruits), one can report it as *Chenopodium* aff. *album* L. where "aff." means affinity. Bohrer and Adams (1977) suggest the use of the term type (i.e., *Chenopodium album* type) but that may be confusing since the type concept refers to nomenclatural type in taxonomic studies where there are specific rules, procedures and categories of types (Ride *et al.* 1985; Voss *et al.* 1983). If the original material is of poor condition but appears similar to the verified sample in the comparative collection, one can cite the identification as *Chenopodium* cf. *album* L. where "cf." means compare (Bohrer and Adams 1977).

This approach using corroborative and associated specimens has been involved in the early development of ethnobotany in the United States and Mexico. During the 1870s and 1880s, Major J. W. Powell and Dr. Edward Palmer obtained seed collections of edible grasses and herbs of the Southern Paiute Indians (Bye 1972). The seeds were collected from the Indian gathering baskets as well as from the prepared foods. This material serves as the voucher specimens. Dr. A. Gray and S. Watson grew the seeds and obtained herbarium specimens from the reproductive plants. This material serves as the corroborative specimen, upon which the identifications were based. Dr. Palmer extended this approach in his Mexican studies (Bye 1979) by collecting plant products at various stages of preparation and in the forms of exchange and storage (e.g., market bundles) while also obtaining herbarium specimens of the plants in the natural condition. In order to distinguish the former specimens from the herbarium collections, Palmer called them "case specimens".

PREPARATION OF VOUCHER SPECIMENS

Each taxonomic group of plants and animals requires special preparation in order to make an acceptable voucher specimen. Many plants and animals have relatively simple methods of preparation in terms of sampling, selecting material, killing, fixing, and preserving the items. Other groups require considerable effort, materials, and equip-

ment. Lee *et al.* (1982: Appendix I) presents a list of references to preparation of major taxonomic groups. It is always best to consult *prior* to the collection of specimens a taxonomic specialist in order to obtain advice and training. It is preferable to seek directions from the repository of the specimens so that the material will meet their specifications.

Apart from the actual preserved organism, the basic data accompanying it (i.e., physically attached to the specimen or to the collection archives) should include (Lee *et al.* 1982:15-16): (1) a unique sample designation such as a collection number for each sample collected at one place and time where each collector or project maintains a sequential, non-repeating numbering system; (2) the position of the sample collection (including: country, state and other political subdivisons; natural geographic location such as river system, sea, etc.; local place name(s); international location designation such as latitude and longitude or Universal Transverse Mercator grid; altitude or depth; and habitat); (3) the date (and time when appropriate) of collection; (4) the name of the collector (and other appropriate donor identification including project, station, and field number); (5) the identity to the lowest taxonomic level, such as to species where feasible; and (6) methods of collection and preparation, where appropriate (e.g., sample as a result of traditional harvesting technique; from cooking pot; from market stand; etc.).

Ethnobiological collections require additional data. Vernacular names should be designated and the language and etymology noted when possible. Specific terms or cultural characteristics should be recorded in the language used to obtain the information; if possible, the original phraseology should be retained. A summary of these data can be provided in the investigator's native tongue. Reference can be made to the name of the informant or collaborator, supplementary or confirming information provided by others, as well as other pertinent data sought in the study.

DEPOSITION OF THE VOUCHER SPECIMENS

A critical element of a voucher specimen is that it be deposited, maintained properly, and available to researchers. Consequently, an investigator should consult the primary repository and the collection curator *prior* to obtaining specimens. Such advance-planning will assure that the sample is adequate for the study and that the specimens will be accepted in the collection and will be useful to the investigator and other researchers. In some cases, an institution may require fulfillment of certain prerequisites such as condition of the specimens, proper collecting permits, payment of handling fee, minimum sample size, etc. Also the investigator will benefit by knowing specifically what and how to collect as well as by having the institution make the contacts with appropriate taxonomists or having the specimens made available to specialists. If the ethnobiologist makes personal contact with a specialist, he/she should be sure that the specimens will be deposited in an appropriate repository (and not in a private collection) or that duplicates of the voucher specimens will be deposited according to the criteria above.

Many ethnobiologists have faced the difficulties of finding museum collections with adequate parts of material for study and comparison. Hence, many of us have generated specimens that would be considered associated specimens as part of individual comparative collections. We have also faced the frustration of curators who do not appreciate these specimens or whose institutions cannot support adquate curation of specimens that do not fit their current practice. Until ethnobiological collections become more acceptable and supportable, what are we to do? First, follow the initial step above—make contact with the taxonomist or curator *prior* to the study. If part of your collections are in a form such that they meet the requirements of current curatorial practices, the curator and institution may be willing to match your donation with an ancillary collection support. If that fails, do not give up on the original voucher specimens—make and deposit

them. Then with the duplicated associated specimens, seek other institutions where your duplicates will be incorporated into the appropriate ancillary collections. If your vouchered associated specimens do not find their way to an official repository, share them with a colleague. The main points are to: (1) deposit your original or corroborative voucher specimens in a proper repository, (2) link them with your associated specimens and (3) have duplicates of the specimens apart from those in your comparative collection available to others. One should realize that curators of museum collections do not receive adequate support or recognition for what they have now. This problem was one of the primary reasons for the Association of Systematics Collections' effort to publicize the importance of such materials (Lee *et al.* 1982).

CITATION OF VOUCHER SPECIMENS

The existence of voucher specimens and the publication of the results based upon the studied organisms are interdependent. Therefore ethnobiological reports in forms of articles, books, and contract reports should include: (1) the scientific name of the plants and animals, and (2) the citation of the voucher specimen(s). In the case of the scientific name, it should be reported at the most accurate taxonomic level possible. Usually the species is accepted as the most useful taxon. Infraspecific taxa (e.g., subspecies, variety, cultivar, etc.) should be used where possible. The specific and infraspecific epithets should be followed by the author in order to complete the scientific name. The voucher specimen must be cited by a distinct identifier which includes: (1) a unique number for the specific item (e.g., collector's name and collection number or accession number assigned by the repository), and (2) an identifier for the repository. This identifier may be the name of the institution and its subunit or collection or, as in the case of major institutions, a code designation or abbreviation. A list of references on repositories for various taxonomic groups and their abbreviations is listed in Lee *et al.* (1982: Appendix II).
An example of a voucher specimen citation is:

Datura inoxia Miller, *R. Bye & E. Linares 13,101*, MEXU. The herbarium specimen of jimsonweed (*Datura inoxia* Miller) was collected by R. Bye and E. Linares, has their collection number of 13,101, and is deposited in the Herbario Nacional of the Universidad Nacional Autónoma de México. By convention (Council of Biology Editors Style Manual Committee 1983), the collector name(s) and number(s), like the generic name and the specific and subspecific epithets, are pinted in italics or written underlined. If one's study includes others' collections, then the collector, collection number, and repository are cited in a similar manner with an exclamation mark (!) following the repository's abbreviation (e.g., MEXU!).

CONCLUSIONS

In ethnobiological publications using names of organisms, the scientific name (to the most accurate taxonomic level) should be reported in addition to the native name(s). The name should be based upon the identification which in turn is documented by a voucher specimen. The voucher specimen serves to verify the identification, to update the identification and nomenclature, and to provide additional data, especially as techniques advance or if the study is not repeatable. Various categories of voucher specimens exist. In addition to the original voucher specimen which may contain ethnobiologically important features but may not have taxonimically diagnostic traits, the corroborative specimen may be required to provide more critical characters. Associated specimens may be useful in forming comparative collections for identifying processed or archaeological biological materials.

The preparation of voucher specimens requires consultation with taxonomic experts and curators in advance of collection. Ethnobiological specimens need basic museum data along with other label data which are physically associated with the specimen or the collection. In order to fulfill their function as voucher specimens, samples have to be deposited at a repository where they will be maintained and will be available to researchers. In archival reports, studies based upon biological organisms should include the scientific name (including the author) as well as the citation of the voucher specimen (which is indicated by a distinct identifier: a unique number of the specific item as well as the name or abbreviation of the repository).

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NOTES

[1]Served as chairman of Characterization of Voucher Specimens Committee for the Council of Curatorial Methods of the Association of systematics Collections.

[2]"Guidelines for Acquisition and Management of Biological Specimens" can be ordered from the Association of Systematics Collections, Museum of Natural History, University of Kansas, Lawrence, Kansas 66045.

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VERIFICATION AND REVERIFICATION: PROBLEMS IN ARCHAEOFAUNAL STUDIES

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ABSTRACT.—Biological materials from archaeological excavations, particularly faunal remains, are of most lasting scientific value if they are properly handled, individually numbered, and adequately reported in published accounts. Archaeologists are urged never to put bones in plastic bags. Authors of faunal accounts are encouraged to publish reasons for their determinations, to mention comparative materials used, and to give individual identifying numbers and element descriptions with their proveniences to aid subsequent study and reverification. Knowledge of the local fauna enhances the validity of interpretations. Better appreciation of the culture and its particular technology would improve biologists' accounts. Archaeological bones have been used in reconstruction of past diets and environments. They are of use to both paleontologists and neontologists. A significant contribution of biological materials is the demonstration of human-wrought modifications (particularly insular extinctions and extirpations) in species densities. Existing comparative osteological collections are weak in quantity and sometimes poor in quality; 82% of the world's *ca.* 9,000 bird species are represented by ten or fewer skeletons in museums worldwide.

INTRODUCTION

The very basis of science is empirical evidence. The taxonomic disciplines are based on specimen documentation, publicly available, subject to reverification. Ethnobiological studies are no exception. The biological specimens that a native consultant associates with her or his ethnotaxonomic identifications (voucher specimens, see Bye 1986), together with that person's critical comments, should be available to future workers once the research is published. Seemingly insignificant or casual comments later prove not to be so, but rather psychological criteria by which a native speaker delimits some ethnotaxon. With the onsaught of cultural homogenization, often the ethnobiologist is doing salvage ethnography, dealing with a small remnant of knowledgable native speakers; there may be no future generation to ask.

While ethnobiological voucher specimens are deliberately collected with supporting ethnographic information, the archaeozoological specimen has been preserved fortuitously, only to be recovered by modern excavators, now increasingly aware of the significance of biological remains. In an earlier stage of the evolution of archaeology as a science, only worked bones—artifactual materials—were considered important. Later, all archaeological bones were analyzed, but only artifacts were saved. With the expansion of contemporary research techniques, it is now realized that *all* biological materials collected from dated proveniences are of potential research value.

I direct my remarks here primarily to vertebrate remains and my examples are mainly from bird bones, with which I work, but the principles apply more generally.[1]

SOME METHODOLOGICAL CONSIDERATIONS

Handling and Packaging. When I asked a number of colleagues what topics to include in this paper, the first and most important response invariably was a plea for the proper

handling of fragile bones by excavators. All other problems pale by comparison. In particular, bird, amphibian, and reptile bones are fragile and easily damaged, especially after years in the soil. More often than not, archaeologists bag in the field bones with lithic artifacts. After preliminary laboratory sorting, bones, often several together, are carelessly thrown into small plastic bags designed for sandwiches, then these "baggies" are all dumped into a paper sack. Finally, the loosely packaged materials are mailed to the faunal analyst. By this time fresh breaks in the bones are usually evident everywhere and often the diagnostic features have crumbled. Gilmore's (1946, 1949) early pleas for better handling of materials are still apropos.

A simple rule: *never put an archaeological bone into a plastic bag*. Each bone should be wrapped carefully in cotton or some fine paper. (A soft paper, 11.5 x 11.5 cm, excellent for this purpose, is commercially available, in rolls, plain or scented; it is available on virtually every archaeological dig.) The individual bones should be packed so one cannot damage another and put into sturdy containers such as small cardboard boxes or plastic vials that are sufficiently long that the well-padded articular ends will not be damaged. The possibilities of the bones being identified to species will be enhanced greatly by this simple procedure.

Each individual specimen should be marked permanently with a unique identifying number, letter, or combination of these. In this way the faunal analyst will recognize the individual specimen when it is laid out in comparative series during identification and can coordinate the specimen later with matching individual data sheets or slips provided by the archaeological team. Any published report should include this individual identifying number as well as the element name so that future workers, wishing to verify a determination, will know specifically which fragment or element had been so identified. Editors overly concerned with space would make this concession to science. The old (but unfortunately not yet fully abandoned) practice of simply putting data-bearing bits of paper in plastic bags or vials with the specimens, or, still worse, attaching paper scraps directly to the bone with rubber bands that eventually disintegrate, is a disservice to science! If each artifact from a site merits an individual identifying number, certainly archaeological bones deserve no less.

A word to the technician cleaning and numbering vertebrate remains: when possible, avoid marking over such critically diagnostic areas as the articulating ends, muscle scars, and tendinal tubercles; bare sections of shafts are the best for numbers.

The archaeological literature is replete with tantalizing early reports of animal remains, often analyzed and reported by workers unfamiliar with the local fauna, unaccompanied by any critical comments or provenience details within a site, and usually lacking specimen numbers. The retrievability of *individual* specimens is critical to science. Many questions may arise not thought of by original workers. Some examples with turkeys, *Meleagris* spp., illustrate this problem.

Steadman (1980) presented a comprehensive study of the subfamily Meleagridinae beginning in the Lower Miocene. The collection numbers and institutional depositions of all recovered and reidentified specimens were included. Ranges were delimited through geologic time to the historic era.

Grayson (1977) questioned the enormous range extension implied by Bedwell's (1971) report of "large quantities of turkey bones" excavated from Connley Cave in the northern Great Basin, dated 3,000-11,200 years B.P. The bones had been identified by well-known ornithologists at a major museum. Because the bones had not been individually numbered and details published, the entire avifauna had to be reassembled and reanalyzed in an attempt to determine which elements had been identified as turkey. Quantitatively, Grayson was able to demonstrate that the reported number of *Meleagris gallopavo* corresponded closely to the number of male Sage Grouse, *Centrocercus urophasianus*, and the general category reported as grouse conformed to the reassembled number

of female Sage Grouse in the sample. Sexual dimorphism had been misinterpreted as taxonomic differences.

Rea (1980) showed that the reports turkeys in the Southwest U.S. applied to two different species, one a paleospecies, *M. crassipes*, the other a neospecies, *M. gallopavo*, appearing locally subsequent to the development of sedentary agriculturalism. However, in the hot low-desert sites occupied by the Hohokam culture, few turkey bones have been recovered (McKusick 1980; Rea 1983:140). Most of those reported (Fewkes 1912; Gladwin *et al.* 1937; Greenleaf 1975) cannot now be found among artifact collections. Apparently few workers have appreciated the potential confusion between turkey and fragmentary elements of Sandhill Crane, *Grus canadensis* (see Hargrave and Emslie 1979).

McKusick (1980) identified three strains of turkey from southwestern archaeological sites, two domesticated, one presumed feral. In completing her monographic work on these forms, she has experienced repeated frustration in relocating reported archaeological bones, often not saved "because they were just turkey" (McKusick, *pers. comm.*).

Humans, directly or indirectly, have been agents in the spread of vertebrates into new areas, as we know so well in our own time from the Common Starling, *Sturnus vulgaris*, House Sparrow, *Passer domesticus*, and House Mouse, *Mus musculus*. Polynesians were aided in their simplification of Pacific island biota by the dogs, pigs, and rats they introduced (Olson and James 1982a, 1982b, 1984). The role of Indians in the spread of animals outside their natural ranges has generally been underestimated; but the archaeozoologist must be alert to the possibility of finding imported species in archaeological sites. Bird trade in the Southwest is now well known through Hargrave's (1970) study of macaws and McKusick's (1976) and DiPeso's (1976) analyses of the Chichimeca trading center in Chihuahua, Mexico. Additional species are still to be reported. Haemig (1978) used ethnohistoric and documentary evidence to explain the presence of the Great-tailed Grackle, *Quiscalus mexicanus*, in the Aztec capital. (Haemig's [1979] interesting suggestion of the jay *Cyanocorax dickeyi* being in Mexico as a descendent of a human-introduced population of the Ecuador-Peruvian *Cyanocorax mystaclis* needs to be checked by comparative osteology; Hardy [1983] noted differences in their vocal repertoire.) In Puerto Rico and the Virgin Islands Olson (1982) has found the extinct rodent *Isolobodon portoricensis* only in the context of cultural deposits, never in Pleistocene cave deposits on these islands. Hamblin and Rea (1985) found rather extensive evidence of bird importation to Isla Cozumel. The Common Turkey, *Meleagris gallopavo*, has been carried aboriginally both to the north and south of its natural range (Rea 1980 and unpublished notes). These are only a few examples of prehistoric peoples spreading animals to new areas, but they should serve to jar archaeozoologists out of provincialism.

Contamination. Healthy debate has been a hallmark of archaeology and in archaeozoology is productive when specimens can be retrieved and the particulars of provenience and association known. An example will illustrate this.

Old World chickens, *Gallus gallus*, became widespread in the New World so quickly that their Hispanic introduction has been questioned (see summary in Gilmore 1950:393-394). Carter (1971) proposed, primarily on linguistic evidence, that the chicken preceded the Spaniards. In places the archaeozoological record appears to support this hypothesis, at least superficially. At the Las Colina site in Phoenix (A.D. 1100-1450, Hohokam), chicken bones were common, but accompanied with Muscovy Duck, *Cairina moschata*, domestic Mallard, *Anas platyrhynchos*, Chinese Goose, *Anser* cf. *cygnoides*, domestic cat, *Felis cattus*, and domestic pig, *Sus scrofa* (Johnson 1981; Rea 1981). At Pueblo Grande ruins, a similar site in Phoenix, I identified chicken, as well as the European Rock Dove (domestic pigeon), *Colmba livia*, in the small archaeofauna. Wasley and Johnson (1965) recovered chicken bones in a Hohokam platform mound in western Arizona, but noted they (as well as a shotgun shell base) were from the bottom of an

area disturbed by pot hunting. At Mayan sites on Isla Cozumel dating up to the historic period, chicken bones (1.1% of the avifauna) were recovered from the top levels of pits (Hamblin and Rea 1979, 1985; Hamblin 1984). In all these cases it is either certain or highly probable that the chicken bones were intrusive. (See Hargrave 1972:6-14 for suggested leads on early chicken research.)

PUBLICATIONS AND INTERPRETATIONS

While papers are often filled with seemingly endless extrapolations and interpretations (anthropologists are not noted for brevity, as a review of journals demonstrates), editors and manuscript referees should insist that faunal analysts mention the related species they considered but rejected in reaching some identification. For instance, does an identification of *Buteo jamaicensis* mean that that was the only species of large *Buteo* the analyst happened to have for comparison? Were other expected species eliminated? Was the analyst aware of the potential range of species that once may have occurred within hunting range of the site? Readers are entitled to this information. Much more convincing would be a statement such as: the tarsometatarsus conforms in characters to *B. jamaicensis*, a species common in the area; the similarly sized *B. regalis*, *B. lagopus*, and *Parabuteo unicinctus* were eliminated on characters of the trochleae; *B. niditus* and *B. swainsoni* were eliminated on size. No skeletons of *B. albonotatus* or *Buteogallus* were available for comparison.

Certain species are of significance because of the temporal occurrence documented by the archaeological context. Many bird species have been extending their ranges northward during historic times (Phillips 1968; Rea 1983:87-90). The discovery of the Northern Cardinal, *Cardinalis cardinalis*, in several archaeological contexts in the Southwest merited careful analysis (Ferg and Rea 1983). The possibility of its being a trade item from the ancestral southern range could not be eliminated. A reader, coming upon such a report without comment and careful substantiation, should indeed be skeptical.

Recently, after five attempts with an anthropological editor from a major U.S. university, I failed to reinstate an introductory sentence in my faunal report to indicate country, location, cultural horizon, and approximate date of the site. The reasons he gave for his veto were that the site was well known (it is on another continent) and if I included such an introduction, then others might want to do likewise, contributing to redundancy in the overall work. Why not? Individual chapters and appendices by faunal analysts are often reprinted and circulated to specialists in the area. Such reports should be comprehensive in themselves, without reference to the whole work.

While archaeologists are often satisfied with a mere catalog of recovered taxa and perhaps a bit of "interpretation" (the amount usually inversely related to the quantity of bone recovered!), biologists may sometimes be faulted for thinking in terms of generic "man" and "his" effects on environments, as if "man" somehow came devoid of all complexities of culture. In the Southwest, for instance, one could not equate the types of habitat modifications resulting from the town-dwelling Pueblo Indians, the nomadic Athabascans, the riverine rancheria Pimas, and the two-village, maximally xeric-adapted Papago bands. Each has a different language and classification system, a different set of taboos, a different relative reliance on hunting, gathering, and agriculture. Even in this arid region, cultures could afford to be selective (Rea 1981). Islanders, perhaps, could not. "Panhuman" explanations of faunal and floral changes and extinctions are too sweeping, particularly where a diversity of technologies existed. L. Marshall (1984:805) has cautioned overkill enthusiasts to view extinction on each landmass as a discrete event. Archaeologists may be able to fine-tune this picture even more precisely in time and space.

Perhaps the most frequent use of vertebrate remains from sites, both paleontological and archaeological, has been in paleoenvironmental reconstructions. While each method of interpreting data has its pitfalls (see the extensive discussion in Grayson 1981), the presence of indicator species in a site will always remain a primary value of archaeozoological studies. The validity of any interpretation depends on two factors: the fidelity of an organism to a particular habitat or microhabitat and the interpreter's familiarity with the behavior and ecology of that organism.

Some species are such generalists or are so mobile that presence in a site yields little ecological information (e.g., Great Horned Owl, *Bubo virginianus*, Coyote, *Canis latrans*). The Red-winged Blackbird, *Agelaius phoeniceus*, and Sora, *Porzana carolina*, may indicate mesic habitats, but do not have high fidelity during migrations, for instance. But the Least Bittern, *Ixobrychus exilis*, and Water Shrew, *Sorex palustris*, are indicators of well-developed emergent or other low surface vegetation in riparian habitats. Suites of species with narrow preferences are ultimately the most informative. Sometimes two closely relates species replace each other in response to habitat alterations. In the Southwest, for instance, the Gambel's Quail, *Callipepla gambeli*, and Scaled Quail, *Callipepla squamata*, are almost invariably allopatric. Degradation of desert grasslands by overgrazing and consequent invasion of thornscrub increases Gambel's Quail habitat at the expense of the other (Phillips et al. 1964; Rea 1973).

Archaeozoologists (and this point is not limited to them) may be quite adept at species identifications in laboratories, but may have very little experience with the species in life. Unless the interpreter has had actual field experience hunting, trapping, netting, or fishing in the appropriate types of habitats, the interpretations will likely be naive at best. (No examples need be cited.) Familiarity with the habitat and the species found there should be a prerequisite for interpretations. Additionally, it would be appropriate if archaeologists in charge of team projects would arrange visits to their site by the biologists who will analyze floral or faunal materials.

More questionable, in my mind, are some of the attempts to extrapolate prehistoric diets from archaeofaunal samples, even though many elaborate models for analyzing bone have been devised. Often only a mere handful of vertebrate remains may be recovered from a site, sometimes because of poor recovery techniques (too coarse screening), more often because very little material was preserved. Without direct evidence of cooking, it is hazardous to equate presence in a site with food or even with human agency. Rodents and reptiles, for instance, find ruins excellent localities for burrows, where some may die. Pot hunters, picnickers, and ranchers may contribute to the faunal assemblage. In my own experiences with peoples on a subsistence economy in the tropics and deserts, dogs seem to be the primary filterers of discarded bones. It is only by accident that anything survives. Overinterpretation of faunal remains is a futile exercise.

Excavated biological material may serve purposes not originally anticipated by the archaeologist. Stanton's Cave in the Grand Canyon, Arizona, was originally excavated to learn something of the enigmatic people who cached there split-twig figurines (the Society of Ethnobiology's logo). But the 43,000-year accumulation of materials recovered in the meticulous fine-screened excavation ultimately proved of greater paleobiological interest (Euler 1984).

Paleontology and archaeozoology may overlap, even in the relatively younger contexts of the New World sites. In the West Indies, for instance, several extinct birds are known only or primarily from middens: a crow, *Corvus pumilis*, a macaw, *Ara autochthones*, and a rail, *Nesotrochis debooyi*. The unidentified *Ara* sp. and *Aratinga* sp. from Postclassic Mayan sites on Cozumel Island are either new species or long-distance imports (Hamblin and Rea 1985). On the Pacific coast Morejohn (1976) recovered the extinct Pleistocene flightless duck, *Chendytes lawi*, from early middens. Extinct species of Hawaiian birds are just being discovered and described, many overlapping with Polyne-

sian colonization. Olson and James (1982a, 1982b, 1984) so far have documented the extinction of 54% of the endemic species of Hawaiian land birds, extinctions attributed to pre-European habitat changes and hunting.

Dated archaeological faunas have always been an important source of specimen documentation for the former distributions of species now extinct, endangered, or geographically restricted. Parmalee has been prominent in documenting former ranges of both birds and mammals in North America (Parmalee 1958, 1960, 1961, 1967, 1971, 1981, etc., Parmalee and Perino 1970).

Perhaps the most significant theoretical implications have arisen from such analyses of vertebrates recovered from islands, particularly those faunas permitting pre-human and post-human comparison. Such work is generating responses that seriously question the baselines used in the voluminous biogeographical studies of supposed fauna equilibrium and "turnover" on islands (see Olson and James 1982, 1984:777-778, Steadman and Olson 1985, Steadman 1986, ii, 88; see also Rea 1983: 14 on deserts).

COMPARATIVE COLLECTIONS: THE WEAK POINT

Growth. Curators and other managers of collections used in faunal analyses should continuously upgrade their osteological collections, both in species numbers and the number of individuals in each series. I know of some analysts who regularly work with only a small number of species represented from their geographic area. One must be skeptical of their scientific results. Collections must grow if they are to be useful research tools, and state and federal permit agencies as well as museum and university administrators must continuously be educated to this fact. Most have never had the practical experience of having attempted identifications. Archaeofaunal analysts might help remedy such ignorance by regularly supplying reprints and copies of reports to agencies and administrators.

Certain researchers, particularly some "field" ornithologists and theoretical ecologists whose studies are unverifiable, have proposed in various recent publications that museums have sufficient numbers of specimens already, particularly of birds. Any working archaeozoologist, paleontologist, or taxonomist knows the falsity of such statements (see Olson 1981). In a worldwide inventory of avian anatomical skeletons, Zusi and others (1982) discovered that about a third of the species of birds of the world (2,706 species) are not represented anywhere by even a single skeleton. Of the approximately 9,000 avian neospecies, over 7,400 (82.3%) are represented by ten or fewer skeletons worldwide. Only 2.1% of the species are represented by over 200 skeletons in the world's museums. Quantitative studies of avian skeletons are virtually impossible. Those who appreciate the use of osteological collections should be aggressive in their conservation and expansion. Only rarely should a specimen be discarded. What may be superfluous in a local collection will likely be missing in some other.

Frequently it is necessary to borrow skeletons to complete a faunal analysis. When possible it is best to request the specific element needed rather than trust an entire valuable specimen to the mails. An updated listing of avian skeletal holdings in the world's major museums is available (Wood and Schnell 1986).

Quality. The mere existence of a skeleton does not assure its utility. The quality of extant osteological preparations in many university and museum collections is often so poor that the materials are nearly useless to the archaeozoologist or paleontologist. Poor cleaning and grease saturation are two major problems. Shot or impact damage and loss of elements through misguided use further reduces the utility of specimens.

Skeletons cleaned by dermestid beetles often have diagnostic features obscured by adhering bits of tendons and cartilage, and they are sometimes more greasy than skeletons

cleaned by other methods. Bacterial maceration usually provides the cleanest skeleton. However, cranial elements usually become badly disarticulated or lost. Such parts as the quadrates, pterygoids, lacrimals (ossa prefrontalia), larynx, syrinx, and sclerotic rings are most useful to phylogenetics if preserved *in situ*. My personal choice of preparation methods is maceration for post-cranial skeletons and dermestids for all cranial parts (including trachea and hyoids). Fat birds are best cleaned entirely by dermestids because the lipids may cause bone disintegration during maceration.

Grease, which may saturate the bone walls from the inside, often renders the normally opaque external surfaces translucent. Working collections must systematically be checked for grease. Degreasing is expensive, time consuming, and dangerous. Ammonia water will sometimes remove light saturations, but will usually take off the carbon ink specimen numbers as well. Heavier saturations usually require lengthy soaking in a solvent such as acetone or gasoline, with rinsings in several progressively cleaner baths.

Bones of young animals occur in archaeofaunal assemblages. This is not surprising because they are seasonally more abundant and often more vulnerable to human predation. The rearing of young birds and mammals is a widespread practice among many North and South American tribes, particularly tropical ones. Nestling birds and juvenals still growing are very rare in osteological collections. Those in charge of building comparative collections should make a special effort to salvage juvenal and young. Hargrave (1970, 1972) was one of the few ornithologists who consistently collected skeletons of juvenal birds, illustrating their diagnostic specific and generic features. Using his materials for comparison we were able to distinguish two species of turkeys even in young not fully grown (Hamblin and Rea 1985).

SUMMARY

It may appear that I have belabored some points here that seem obvious. Would that this were so. The archaeologist who walks into my laboratory with a bunch of unprotected, unnumbered bird bones in a "baggie" is the rule rather than the exception. Reports that give no collection numbers and fail to mention how an identification was arrived at or what species were compared still continue to be published, but are becoming fewer.

If archaeozoology is to maintain itself as a science, the *specimens* on which it is based must be permanently stored and retrievable for reverification. The scientific value of dated faunal materials may far exceed that originally envisioned by the archaeologist. Biologists have much to learn from archaeologists to the betterment of their studies and vice versa. In general, a true synthesis of the disciplines is a goal still to be achieved.

NOTE

[1]Drs. David W. Steadman and Storrs L. Olson suggested critical topics for discussion in this paper on the basis of their extensive experience studying biological materials from archaeological sites. David Steadman and Philip Unitt kindly reviewed the manuscript, making various suggestions for its improvement. Authors who have kept me supplied with reprints will find their papers cited most frequently here.

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FOOD SAMPLE COLLECTION FOR NUTRIENT ANALYSES IN ETHNOBIOLOGICAL STUDIES

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ABSTRACT.—Many ethnonutritional studies have suffered from lack of precision and poorly developed skills in data collection, sample collection, labelling of containers, and advanced planning. This paper addresses some of the practical concerns facing ethnonutritional field workers and suggests ways and means to enhance accurate transmittal of information to laboratory workers, many of whom often have minimal or no field exposure.

PURPOSES OF NUTRIENT ANALYSES IN ETHNOBIOLOGICAL STUDIES

If all of the foods in local indigenous food systems were represented in tables of food composition originating from national laboratories and data banks, it would be unnecessary to conduct basic chemical work to gain insight and understanding into the contributions of such foods to the nutrition and well being of those consuming them; indeed, this paper would then be superfluous. This is not the case, and these studies, including the field work which must precede the chemical analyses, are needed if we are to gain a full understanding of the diets and nutritional status of contemporary, historical, and prehistorical people.

Nutrient analyses are needed to complete food composition data bases which are then used with quantitative dietary data to assess individual or group nutritional health status. den Hartog and van Staveren (1979) have described the various methods of assessing dietary intake and, except to emphasize that both careful planning and statistical treatment are essential in overcoming inherent difficulties in quantitative food intake assessment, these procedures will not be dealt with herein.

In addition to its use in defining quantitative nutrient intake of people, food composition dxta is useful for its own sake when an ingested item has not been previously studied and it is known to be consumed frequently and/or in reasonable quantity by a group of people. Nutrient analytical work on foods helps to identify particularly superior or inferior potential new foods for horticultural or genetic studies. It contributes to the body of knowledge of how indigenous people met their nutrient requirements, and helps to fill the gap in our understanding of the intrinsic worth of indigenous diets and the complexity of effects modern dietary adaptation has on the health of native people. The importance of expanding our collective knowledge of the use and chemical properties of indigenous foods has been noted many times (Behar 1976; Kuhnlein 1984; Turner 1981). This knowledge is particularly valuable for native or cultural groups who wish to promote cultural avenues for health improvement and self-care. Providing information for wilderness survival training programs, and stimulating markets for new ethnic foods native people may wish to promote as cottage industries (Kuhnlein 1985), further justify recording nutrient data in indigenous foods. It is perhaps worth mentioning that the

techniques and methods presented herein can also be interpreted for work on the toxicological and medicinal properties of ingestants.

SAMPLE IDENTIFICATION AND DEFINITION

The hallmark of superior sample collection in ethnographic settings is the elicitation of accurate knowledge about the identification and preparation of the food samples by the local people. This expertise needed for good data collection is often not appreciated and is perhaps the most underrated skill in the entire chain of research events—a process which begins by identifying the groups and foods of interest and ends with collaboration with laboratory scientists and publication. Native or ethnographic "consultants" or "informants" play a central role in generating accurate data—they must be well-informed of the variety of practices in the community and must be capable of trust to relate it accurately.

Many native people are highly motivated to share their wealth of knowledge for the ultimate purposes of recording it in the written media for the posterity of their groups (Van Asdall 1985). They exchange the trust for accurate knowledge by the data collector with their own trust that the information they give will be recorded honestly and presented in the best interest of their people.

Tales of mismotivated consultants or informants are legion among ethnographic researchers. Some will provide any type of information or samples they perceive as desirable for money, as long as the funds are available. Others are reluctant to share what they feel is private family or group information, and will deliberately side-track an investigation. Yet others will insist their particular method of preparation is superior, and may ignore the techniques of others in the community. Because of these potential deterrants to good data collection it is wise to gather information and samples from several reliable sources in the community.

The involvement and training of native people to collect accurate data and samples is a key component of any health promotion effort in contemporary indigenous settings. If they know the rationale and procedures used, native people are highly motivated to report results back to the community, and thereby stimulate local interest and the impetus to use the results for health promoting activities. It is clearly understood that the investigator must develop a reputation for returning the results of all data collection and sample analysis to the community and that this is requisite to the research and publication process.

As reported elsewhere in this issue (Bye 1986; Rea 1986), the identification of ethnobiological samples with voucher specimens needs to be thorough and as complete as possible. Ideally, taxonomic identifications are made in the field with the help of a local botanist or zoologist. In the absence of this assistance, dried samples (plants) or detailed photographs of an intact entire individual organism will assist identification, and should be accompanied with the local linguistic terms and any other common language names that are used.

In the case of food samples, it may be pertinent to the investigation to sample the single raw species, as well as the final prepared product, since multiple processes and ingredients obviously modify the original nutrient content. Assurance that the sample is representative is necessary. Depending on the product in question, this can be done in a variety of ways: picking berries from several bushes in an area; thoroughly mixing a large quantity of product (for example, a large bowl of ground corn meal) before extracting the sample size needed; if a representative animal tissue is desired, taking portions from several muscles that are commonly eaten. In the latter case, the desirability of single muscle or organ tissue sampling must be carefully considered in lieu of mixed amounts of the total edible portion of like tissues. The sampling of ethnobiological food

species in the field is as hazardous as getting good ethnographic data from "consultants", especially if the species are limited in quantity or labor intensive, and the "consultants" would prefer not to part with the amount needed for a thorough chemical analysis.

Whenever possible, the sampling strategy should be thorough enough to permit the desired statistical treatment. However, it is easy to allow statistical considerations to escalate the sample number so high that the costs of analyses are prohibitive. Caution is therefore needed in the original design of the project so that questions that are practical and answerable are put forward.

In addition to sampling the basic food species, complete information on other techniques or ingredients used by other cooks should be defined, if they are not a part of the original sampling strategy. For example, Hopi piki bread is a finished food product which utilizes only four ingredients: corn meal, a culinary ash, water, and a small amount of cooking fat. One could select the most common preparation from First Mesa, for example, and take 4-5 samples of (1) blue corn meal, fine ground, (2) ash prepared from chamisa, *Atriplex canescens*, and (3) the finished product. From these (and published tables of water minerals and marketed fat nutrients) a definition of nutrients contributed by the ingredients could be made, with appropriate statistical tests. Descriptive information to round out the data collection could be made by interviewing 8-10 different piki preparers for: (a) Other types of corn used for piki (white, red, etc.). (b) Other types of ash used (bean plant, juniper) or baking soda. (c) Different recipes or procedures evaluated with measures or balances. (d) Different grinding implements used for the corn which might contribute flavor or mineral additions. (e) Other types of cooking implements: griddle vs. piki stove. (f) Alternate sources of cooking fat: oils, lard, seed oils, animal spine fats, etc.

To thoroughly sample each of these variables would result in more than 100 analyses, a prohibitive amount. Even to sample just the finished piki resulting from several of these variations would be prohibitive, and the difficulty to evaluate just one variable arises.

Although these considerations present a tremendous deterrent to the field investigator sampling ethnobiological food samples, keep in mind that even rudimentory sampling of finished products may give important results that provide the impetus for further investigation. For example, if all piki bread samples made with ash have high levels of calcium, (and they do), and the local population consuming it did not traditionally use milk (they did not) this is a significant find in ethnonutritional research.[1]

Further to these considerations on sampling in food preparation, care should be taken to note precise geographic locations, the individuals involved in sampling and any unusual soil or climate conditions. Careful notes made on the day of collection and ordering of samples is indispensable, especially when multiple samples are taken. Containers are best labeled with permanent ink on the container itself (not the lid, if it is removable); if stick-on labels are used for samples to be frozen, insure that the labels adhere at the desired temperature.

SAMPLE TREATMENT FOR SPECIFIC NUTRIENT ANALYSES

Harris and Karmis (1975) presented a guide for understanding general nutrient stability after standard processes of food preparation and preservation (Table 1). Heat treatment during cooking of any form, canning, and in some instances drying, causes destruction of the greatest number of nutrients. Exposure to air or oxygen, as in drying, dry roasting, or fine dicing or pureeing of foods also presents several nutrients for destruction. Maintaining food products at neutral pH and away from direct light is protective for several nutrients, especially the vitamins. Reducing the pH below 7, as in pickling, fermenting or acidification with vinegar-based dressings is destructive to the Vitamin A

and carotene complex, folate and pantothenic acid. Raising the pH above 7, as occurs with adding baking soda or ash products to grain foods is destructive to thiamin, riboflavin, ascorbic acid, vitamin D and essential fatty acids. A quick review of this summary table

TABLE 1.—*Stability of Nutrients.*

Nutrient	Effect of pH			Air or Oxygen	Light	Heat	Max. Cooking Losses
	Neutral pH 7	Acid <pH 7	Alkaline >pH 7				
Vitamins							%
Vitamin A	S	U	S	U	U	U	40
Ascorbic acid (C)	U	S	U	U	U	U	100
Carotene (pro-A)	S	U	S	U	U	U	30
Cobalamin (B-12)	S	S	S	U	U	S	10
Vitamin D	S	S	U	U	U	U	40
Folic acid	U	U	S	U	U	U	100
Niacin (PP)	S	S	S	S	S	S	75
Pantothenic acid	S	U	U	S	S	U	50
Pyridoxine (B-6)	S	S	S	S	U	U	40
Riboflavin (B-2)	S	S	U	S	U	U	75
Thiamin (B-1)	U	S	U	U	S	U	80
Tocopherol (E)	S	S	S	U	U	U	55
Essential amino acids							
Isoleucine	S	S	S	S	S	S	10
Leucine	S	S	S	S	S	S	10
Lysine	S	S	S	S	S	U	40
Methionine	S	S	S	S	S	S	10
Phenylalanine	S	S	S	S	S	S	5
Threonine	S	U	U	S	S	U	20
Tryptophan	S	U	S	S	U	S	15
Valine	S	S	S	S	S	S	10
Essential fatty acids	S	S	U	U	U	S	10
Minerals	S	S	S	S	S	S	3

S = stable (no important destruction).
U = unstable (significant destruction).

Adapted from: Harris and Karmis, 1975.

will guide planning of which nutrients are most effectively determined in prepared or preserved food products. It can also guide sampling and storage procedures.

An important procedure in food sampling, often overlooked in field collections, is thorough cleaning of the foods before packaging for shipment to the laboratory. Washing for removal of soil particles is especially important if the food is destined for mineral analyses. Microbiological assay for vitamins are sometimes upset by heavy microbe contamination of foods, a problem minimized through cleaning. If possible, it is best to sample food items only for the edible portion (EP), which means removing of husks, shells, skins, etc. This is most important if samples are to be fresh frozen and then thawed in the laboratory, since nutrients in the non-edible portions may migrate to the EP during the freeze-thaw process, and give an erroneous picture of what is normally consumed in the home setting.

Although washing of the food product is important, the drying of the product before storage is equally critical, since excess water clinging to frozen samples will be inseparable from the nutrient containing EP after thawing and will therefore dilute the nutrient content, giving unrealistically low values. This consideration is especially salient for foods with initial low values of the nutrients in question.

Sampling food products in field settings and storage under field conditions is to be considered carefully before deciding what analyses are practical. If mineral analyses are the objective, simple drying procedures can be done in the field, using open air/sun drying or gentle oven temperatures (not more than 100°C). Samples need to be protected from contamination by other minerals, as given by metal implements or storage containers, but otherwise packaging is simply done in clean (unused) plastic bags. Drying also prevents shipping excess weight in the form of water. Freezing at -10°C in airtight plastic containers is needed for storage of samples destined for analysis of most vitamins. For most food, samples should be taken from the EP of the fresh state or from a freshly prepared product ready for consumption. Vitamin E is the only nutrient for which freezing is known to cause significant losses. Otherwise, all nutrients including minerals can be assayed from freshly frozen food products.

Bye (1986:1-8; this issue) reports on recommended, and, in some cases, standardized procedures for connecting field notes, voucher, corrororative, and associated specimens of plants and animals. Some or all of these may ultimately be deposited in different institutions. Although record keeping in field ethnonutritional studies have not yet been formalized, field logs can serve many important functions, e.g., they help to orient the investigator when several shipments are sent from the field to the laboratory and, along with logs of sample containers and storage conditions both during transit and in the laboratory, are useful in sorting out problems of anomalous nutrient values in similar food products. Information of this sort can best be transmitted to the laboratory staff in a detailed "transfer letter" enclosed in the storage container explaining what samples are arriving from the field and to what conditions they have been exposed before (and during) shipment. This document might also specify dates of sampling and shipment, cleaning process, treatments, as well as actual names of the foods with their code numbers so that information and interpretation can be related, if and when necessary, to field logs.

Within the laboratory, records should be kept of the length of storage time, storage conditions and frequency of freeze-thaw of samples. Canned and frozen foods are known to lose value for several nutrients with duration of storage and fluctuations in storage temperature (Erdman and Erdman 1982).

The techniques of analysis of the food samples are best predetermined with the laboratory staff prior to field collections so as to avoid errors and unnecessary mishandling of samples and potential nutrient destruction in the process. At the same time, the actual size of sample needed for the various analyses should be determined to avoid

undersampling and the consequent need to delete duplicate measurements which insure validity and reliability of the assays. For some nutrients, pretreatment in the field will enhance nutrient stability (for example, liquid food products destined for folate analysis could have a known quantity of crystalline ascorbate added to preserve the folate content before packaging). Methods outlined in the JOAC (Horwitz 1980) or in Southgate (1974) give general guidance to consideration of sample sizes for particular nutrient analyses. Greenfield and Southgate (1985) present a thorough review of record keeping and laboratory procedures needed for quality food data.

A note of caution is needed for the interpretation and comparison of nutrient results when methods of sampling and analyses are not consistent. The compilation of nutrient composition tables from multiple data sets are especially problematical when consistency has not been maintained. It takes an experienced chemist to decipher variance in data when analytical methods differ.

NOTE

[1]For more information on the nutrient content of piki bread, see Calloway, Giauque, Costa (1974), Kuhnlein, Calloway and Harland (1979), Kuhnlein and Calloway (1978), and Kuhnlein and Calloway (1979).

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A World Directory of Ethnobotanists. S. K. Jain, P. Minnis and N. C. Shaw. Society of Ethnobotanists, Lucknow, India. 1986. Indian Rs. 15; U.S. $2.00; L 1.50.

The Indian Society of Ethnobotanists, established in 1980, has just published a most useful directory, one that will greatly strengthen the links that bind the many specialists in interdisciplinary fields tangential to economic botany and the sundry aspects of ethnobotany.

This valuable contribution, listing the names, addresses and usually the specific research interests of 489 ethnobotanists, can be purchased by writing to Dr. Ved Prakash Tripathi, B-20 CRIR Colony, Nirala Nagar, Lucknow, India.

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GUIDEPOSTS IN ETHNOBOTANY

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ABSTRACT.—A review of the history of a natural system of maize classification is provided. Detours in the archaeological analysis of maize abound. Tests suggest the extrapolation of cob row number from kernel angles lacks predictive value. Flotation analysis is forging ahead. Current trends in regional studies might be better integrated with anthropological model construction or destruction. Charcoal identification merits improvement through use of metallurgical microscopes coupled with more rigorous use of plant anatomy. Analysis of leaf epidermal fragments in coprolites broadens the range of information recovered. The biotic factor, including the anthropogenic one, needs to be considered as seriously as climate and soil in the formation of prehistoric and modern plant assemblages. Ethnobotanical interpretation of prehistoric plant remains benefits by insights into the kaleidescopic patterns formed by modern and historic interrelationships between plants and man. It is never too late for contemporary studies. Museums rather than colleges or universities continue to provide workspace for ethnobotanists. Suggestions are given in regard to academic education in ethnobotany and exemplary books and reports in the field.

INTRODUCTION

GUIDEPOSTS IN ETHNOBOTANY

Ethnobotany is flying high in the 1980s. The first issue of the *Journal of Ethnobiology* was published in 1981; the first general session on ethnobotany at the annual meetings of the Society of American Archaeology was in 1983; and in May, 1985, after settling into my seat on Southwest Airlines Flight 192, I was pleased to notice that the cover story of their flight magazine featured the contemporary ethnobotanical work of Dr. Richard Felger in northwestern Mexico and southern Arizona. That same August, *Forbes* magazine featured an article on the applications of pollen analysis (Teitelman 1985). Apparently ethnobotany has emerged from the low visibility of a pioneer discipline into a new phase of prominence.

Because your good editor suggested a historical perspective might be valuable, I wrote the first two sections on Maize and Flotation chronologically, but my initial resolve to follow this approach lessened by the time I reached the next section on Charcoal and Fecal Analysis and evaporated by the time I wrote Changes in Plant Life. Do not be misled by these initial digressions into history, for the pages that follow have taken another more personal orientation. I have participated in ethnobotany from the time when it comprised a handful of people, and witnessed with mixed emotions a series of changes in the field. The following pages typify my rejoicing over examples of fine work and brooding about areas that seem deficient. I regret that I can not know and reference all the best efforts in ethnobotany. On the other hand, I find it judicious to avoid citing what is less than satisfactory. The recent increased prominence of ethnobotany carries an increased responsibility toward producing a quality product; some sort of guide seems especially necessary right now. Lest you misunderstand, be assured that I admire the achievements of those who have pioneered in learning about maize, flotation, charcoal, fecal analysis, environmental reconstruction and ethnobotanical interpretation. Nor are

you to assume that I am setting myself up as an oracle for ethnobotany. I expect some disagreement with my judgment. A discourse such as this will perhaps stimulate a dialogue for improvement within the profession so that we can avoid, in spite of the meteoric growth of our discipline, a hollow center leaving us too vulnerable to withstand criticism from outside.

MAIZE

Southwestern Pueblo Indian maize was among the first to be carefully sampled and classified according to a natural system (Anderson and Cutler 1942). A natural system relies on the ability to establish genetic relationships and trace origins. In the course of Anderson's self-directed study of maize he learned to depict variability important in the analysis of introgressive hybridization by means of a pictorialized scatter diagram (Anderson 1946:175). In the first version developed for southwestern maize (Carter and Anderson 1945) each point on the scatter diagram registered index numbers that summed data rather than recorded the primary measurements on each plant. In Anderson's subsequent publication (1946) the system was perfected by employing row number on the *x*- and kernel width on the *y* axis. The small circle or dot representing the intersection of values was proportionately elongated to illustrate the amount of kernel pointing, and shaded to correspond to the degree of kernel denting. Both row number and kernel width were used because they were end products of many genes acting in concert. For example high row number, narrow kernels and pointed kernels tended to be inherited together. By choosing single characteristics that were the expression of multiple genes, Anderson could sit in a farm yard and measure the salient features of a cob by making a single mark on the appropriate intersection of his scatter diagram which represented all three observations simultaneously. He could average the data for all maize ears in a village. Later, one could derive average values for all villages belonging to a tribe, and for all tribes belonging to a linguistic group. With such economy of effort not only Pueblo Indian maize but all maize of Mexico could be classified in the hope of tracing its northward migration.

In the 1930s long before Anderson developed an interest in maize, Carl Sauer, Professor of Geography, University of California at Berkeley, became interested in documenting the varieties of corn grown by Indian tribes in remote locations in northwestern Mexico. Isabel Kelly, one of Sauer's students, provided Anderson with critical collections of chapalote corn from Culiacan and Sinaloa and of the related maize reventador (Anderson 1944) from Jalisco and Zacatecas. Both races proved an essential modern link in understanding prehistoric maize in the southwest. Anderson combined his keen perceptions of morphology with his special interest in multiple gene complexes to provide informed speculation that maize reventador represented an old and primitive race (Anderson 1944:309). Later, the related chapalote race was thought more important (Wellhausen *et al.* 1952:57). The magnitude of Anderson's contribution to the understanding of maize in Mexico is apparent when we realize that ten of the races he delineated were incorporated into a total of 25 races recognized for Mexico (Wellhausen 1952:13).

When the descriptions of Mexican maize races were published in 1952 their correlation with certain patterns of distribution of chromosome knobs were known. For example, all those called "ancient indigenous races" had low chromosome knob numbers. Since then an outstanding team of maize cytologists undertook a study of nearly twenty years in mapping the distribution of chromosome knobs of most maize races in the Western Hemisphere as well as the majority of races of teosinte, the closest relative of maize (McKlintock *et al.* 1981). They were able to demonstrate that some races considered relatively ancient from the Pacific coast are related to each other with the 7s large knob: nal-tel, zapalote chico, chapalote, reventador and harinoso de ocho (McKlin-

tock *et al.* 1981:40). Through many pages of carefully documented analysis the team was able to deduce two routes of entry of maize into the Southwestern United States—one along the Pacific Coast and a second that apparently ran from central Mexico northward to the Rio Grande. Although Barbara McClintock won the 1983 Nobel Prize in medicine for discovering transposable elements based on the mode of inheritance of color in corn kernels and leaves, ethnobotanists will remember her leadership in independent cytological corroboration of the integrity of the races that Anderson first deduced from maize morphology. The finer relationships between races revealed in the study she headed should sustain an interest in racial classification for some time.

Given the statistical advances applied to maize variability since Anderson's initial work (eg., Goodman and Paterniani 1969, Bird and Goodman 1977), and given that cupule width represents the nearest measure of kernel width when the cob alone is considered, it is a tribute to Anderson's keen perception for the authors of a recent technical report on maize from the Peruvian coast to remark that of the seven characters used, row number and cupule width were the two most consistently useful traits in distinguishing maize types (Bird and Bird 1980:330). Studies in the 1980s have corroborated the early methods used to identify races, singled out the most ancient races of maize, and given the scientific world a fresh explanation of the seemingly abrupt transition from teosinte to maize as a catastrophic sexual transmutation (Iltis 1983; Gould 1984). The theory has not gone unchallenged (see Galinat 1985) but the persistent storms over maize evolution that have raged for decades have lessened in intensity.

In the Southwestern United States the foot and leg work necessary to provide information on the prehistoric distribution of maize types was done primarily by Hugh C. Cutler in cooperation with field archaeologists. In Cutler's many appendices to archaeological site reports he provided data on cob row number and cupule width. What his colleague Anderson learned of modern races in Mexico and what Cutler himself learned about southwestern corn could by synthesized to interpret prehistory. Two appendices are particularly rich in insights to the linkage of history and prehistory (Cutler 1964 and Cutler 1966). In the earlier report Cutler relates the maize from Carter Ranch Pueblo to the Pima-Papago race that he and Anderson first defined, to more recent racial definitions, and to his observations on Hopi maize. In the later report he provided a greatly expanded overview of southwestern maize as part of his Glen Canyon, Arizona research (Cutler 1966:12-15). He presents views on the progression of races of maize in the southwest and the role of Mexican races like ***onaveño*** and ***harinoso*** de ocho in prehistory.

Maize has provided us with an incredible number of detours in our thinking before we have found the main road. Differences of opinion about which is actually the correct road disappear only in retrospect. Cobs, kernels, pollen, phytoliths and radioactive isotopes have all been the focus of considerable debate, and not without reason. While cobs are the most efficient way to gather data about prehistoric corn, the process is far from perfect. Burned cobs seldom represent more than a random cross-section of a cob devoid of kernels. Although adjustments for cob shrinkage can be made, the lack of attached kernels deprives the investigator of ancillary criteria to assess the racial affiliations of the population formerly present. In addition, corn grown under stress may have reduced row number (Emerson and Smith 1950 in Cutler 1966:11). Recently Mackey (1985) has advocated the use of maize to demonstrate stress in the Gallina Llaves Valley, New Mexico archaeological sites. A necessary prerequisite of such a study is establishing the existence of a uniform race through time. Real problems exist in establishing the former existence of any single race (Benz 1985).

A modern statistical treatment of the races of maize in Mexico and the southwest need development to serve as a template against which prehistoric populations can be fitted. The loss of a host of criteria in archaeological maize makes it more difficult to accurately fit the narrow racial categories formed from intact modern maize (Wellhausen

et al. 1952) rather than to the more broadly defined races first delimited (Anderson and Cutler 1942, Anderson 1946). We still do not know to what degree changes in southwestern corn result from cultural preferences or selection and which are products of diffusion of new races of maize. Ford (1981:13) has argued that from the genetic variability present in southwestern corn by 300 B.C. one could derive the morphological variability seen in subsequent strains through natural and cultural selection. Ford's ideas in conjunction with the evidence for prehistoric diffusion of harinoso de ocho to the mid-west (Galinat and Gunnerson 1963) might well be considered once the boundaries of our racial taxonomy are in better order and the accuracy of our dating verified with direct radiocarbon dating.

To further complicate matters the process of extrapolating row number from prehistoric maize kernels (Cutler 1956) appears to have serious problems, despite seemingly impeccable theoretical grounds. The sides of the kernel on a fully developed ear should be compressed by the limitations of the space available on the nearly circular cob (360°). The circle could be divided into 8, 10, 12 or 14 parts corresponding to the number of rows on the cob. A cob of 8 rows should have kernel side angles of 45°, 10 rows 36°, 12 rows 30°, etc. If these assumptions were correct and one had only kernels to measure, the angles of the kernel sides could give an estimate of the row number. The accuracy of the kernel angle method of determining row number was tested recently by Pearsall (1980) when a series of 25 kernels from cobs of known row number were measured. The tendency of kernels from 8 and 10 row cobs to be confused and the inaccuracies in the assessment of cobs of 12 and 14 rows were noted.

In a 1984 workshop on ethnobotany in Tucson I directed a similar test on the predictive value of maize kernel angles for row number. Two modern kernel covered ears were selected, one of 12 rows called "A" and one of 14 rows called "B". Each was checked to verify that the ear had the same number of rows at the base and the tip, and the kernels were then removed from the cob. Without knowing the original row number, each student took 30 grains, measured the angles formed by the kernel sides, then classified the kernels as to 8, 10, 12, or 14 rows. Our results (Table 1) show little relationship to actual row number. A class member more experienced in measuring maize kernel angles who used a metal template instead of ruled angles produced similar results. Our obvious conclusion was since we could not estimate the true row number when we could verify the accuracy of our work, there was no point in applying the method to archaeological kernels which were apt to have suffered additional modification of the kernel angle through carbonization.

Current techniques on how to recognize maize pollen in the United States now seem in need of revision. North of Mexico palynologists rely upon the large diameter of maize pollen as a trait sufficiently unique to separate it from other grass pollen. In contrast palynologists in Mexico use features other than size to distinguish maize from the closely related teosinte. In the late 1970s when little barley grass *(Hordeum pusillum)* caryopses

TABLE 1.—*Row number determined from kernel angles on two ears of maize.*

Ear*	Row No. 8	10	12	14	Total kernels
A	36	76	54	6	172
B	13	72	69	11	165

*A was 12 rowed and B 14 rowed.

were identified in the Southwest (Gasser 1981), palynologists showed legitimate curiosity as to the diameter of this New World cereal pollen. Early literature (Jones and Newell 1948:141) and independent measurements by southwestern palynologists confirmed that the diameter of little barley grass resembled most grasses. Recently, however, while doing background research on native barley I noticed that *Hordeum pusillum* from Granite Reef Dam, Arizona, produces larger pollen (Covas 1949:14). In addition, *H. arizonicum* bears pollen grains with modal pollen diameters of 60-68 micrometers (Covas 1949:17), which are very similar to chapalote maize pollen (Irwin and Barghoorn 1965). If the hexaploid nature of *H. arizonicum* (Rajhathy *et al.* 1964:196) gives it certain competitive advantages in growing in modern Papago fields as Nabhan has observed (1983:200) it may have done so in the past. Arizona barley grass has been reported in Maricopa, Pinal and Pima countries (Gould 1951:108) in the Hohokam culture area. We have chanced upon one species that locally negates the assumption that the size of maize pollen produces a unique signature that differentiates it from other grasses. Perhaps systematic research into the size of pollen of all polyploid grasses should be undertaken.

The acquisition of the perception that there is no unique pollen size signature for maize except where local plant geography allows, lets me view with concern another struggle now underway to revise the unique morphological attributes by which maize phytoliths can be recognized (reviewed by Rovner 1983:249-251). As much as we might desire to establish a universal "truth" about maize phytoliths, it might be more instructive to concede that truth is relative. Like maize pollen identification, the validity of maize phytolith recognition may be relative to the amount of ambiguity provided by phytoliths of other plant species growing in the area. Schoenwetter (1974:298-301) has supplied some thought provoking remarks on how one grapples with uncertainties in the maize pollen record in the heart of Mexico. His discussion might as easily apply to related problems with phytoliths.

Although radiocarbon dating began around 1949, it was not until 1967 that the specialists realized maize dated younger than predicted (Bender 1981) because it was one of a number of grasses (Walker and Lewis 1979) and herbs which had a photosynthetic path (C-4 instead of C-3) that accumulated radioactive isotopes differently. Since then the knowledge has been used to advantage in the analysis of the $^{13}C/^{14}C$ ratios in human bone collagen. In animals the isotope values clearly reflect the isotope ratios in their diet, that is, the proportion of C-4 to C-3 plants. Where succulent plants like cholla cactus *(Opuntia)* are eaten, a crassulacean acid metabolism (CAM) mimics the C-4 isotope ratios (Troughton *et al.* 1974). Where maize is likely to be the only C-4 plant consumed, the ratio provides an indication of the importance of maize in the diet, as in Hopewell agriculture (Bender 1981). In the southwest the consumption of C-4 plants such as *Amaranthus, Panicum, Portulaca,* members of the Capparidaceae *(Cleome)* and Chenopodiaceae *(Chenopodium)* contribute to obscuring the vexing question of the former importance of maize agriculture. Studies of its antiquity will benefit from accelerator radiocarbon dating, recently used to advantage in dating individual kernels.

FLOTATION

Although in the 1930s Volney Jones used flotation to recover vegetal remains from adobe bricks from historic Awatovi (Griffin 1978:17) as did Hendry and Bellue (1936) from different historic buildings, the method lay unappreciated by others. In the 1950s Dr. Hugh Cutler visited the Field Museum excavations at Tularosa Cave and Higgins Flat in New Mexico and demonstrated that plant remains could be recovered by throwing screened dirt in a bucket of water and skimming off the floating charcoal and seed (Watson 1976:79). Approximately a year later, when Dr. Cutler visited the University of Arizona field school at Point of Pines (Director: Dr. Emil Haury), a similar

demonstration had portent for future ethnobotanical research. Ten years later, in 1964, Dr. Haury creatively applied Cutler's flotation to the second excavation of Snaketown. There flotation was metamorphosed from a dirt sample scattered on a bucket of water to a fully conceived sampling plan for trash mounds and pits to obtain a chronological series for flotation analysis. Haury developed and directed the method of concentrating the charcoal (Bohrer 1970). As an ethnobotanist my job began with the receipt of the bags of charcoal and seed.

Since the early work on flotation at Snaketown, our understanding of the formation of the carbonized vegetal record and the fraction that is apt to be recovered by flotation has undergone considerable evolution both in North America and abroad. Flotation techniques in vogue in the mid 1970s have been reviewed (Watson 1976) and numerous variants have been reported since then. Research designs compatible with research objectives have been promulgated by Adams and Gasser (1980), Bohrer and Adams (1977), and Toll (1984). Schaaf (1981) has advocated the use of a sample splitter among other techniques, and methods have evolved to check the accuracy of flotation (Wagner 1982). A concommitant development has been the acknowledgement that carbonized seeds differ in their buoyancy and that certain conditions or properties of seeds may cause them to sink (Pendleton 1983). Such problems may eventually lead ethnobotanists who haven't yet done so to the examination of the heavy fraction or to adopt a modified form of water screening, as has Schaaf.

The recent publications of the results of two large archaeological projects at Dolores, Colorado and in Chaco Canyon, New Mexico incorporate several innovations helpful in flotation analysis. At Dolores, archaeologists have formulated criteria for field recognition of four site abandonment modes (Breternitz 1984:166) (1) leisurely abandonment implying minimal preservation of artifactual botanical material; (2) deliberate abandonment, burning within 5-10 years afterward, providing moderate preservation potential; (3) ritual abandonment; (4) catastrophic abandonment allowing optimal preservation of materials. A number of advantages are apparent from this procedure. The segregation of sites in terms of potential for plant preservation helps set priorities in the analysis of pollen and flotation samples. Furthermore the method provides a distinct aid to interpretation, for the manner in which case 2 and case 4 are treated, are vastly different (Halley 1981; Minnis 1981). Finally, in a project of long duration, the prospect remains of increasing the accuracy of judgments made in the field from the results in the laboratory.

At Chaco Canyon, Mollie Toll (1984) undertook flotation from two sites using different sampling designs. With the help of a third site, she has been able to bring into focus underlying patterns obscured by different seeds densities in flotation samples. Her innovations in dealing with unburned seeds may assist those working in other arid areas or historic sites where uncarbonized seed is seemingly still preserved. Two were shallow village sites subject both to erosion and alluvial deposition. A third site (Pueblo Alto) had living surface over a meter deep. Pueblo Alto was sampled like one of the shallow village sites and used to evaluate the possibility that some of the unburned seed might be prehistoric. The pattern of seed recovery from habitation room to habitation room at Pueblo Alto varied little. Weedy *Chenopodium*, *Amaranthus* and *Portulaca* were carbonized in heating features and uncarbonized on floors in decreasing density with increasing distance from food processing features. The distribution suggests prehistoric primary deposition of unburned seeds of Pueblo Alto. In the shallow sites the conclusions drawn by using only burned seeds or burned plus unburned seeds are similar—a pattern of using immediately available disturbed ground species in company with agricultural products.

There are several qualifiers in the above study. The first is that the difference between burned and unburned naturally black seeds can be subtle. Even slight parching

of a seed coat may deter degradation. The second is that high standards of sample selection need to be exercised rigorously. Sample locations that were cleaned out prehistorically and filled with alluvium, that were disturbed by rodents, that were ambiguous in terms of the source of cultural versus natural deposition (like post holes) had to be eliminated from consideration (Toll 1981:2). Unless non-cultural factors active in archaeological deposits are recognized and systematically excluded before flotation samples are analyzed, Mollie (1984:249) warns that "Non-cultural factors may introduce more variability into the archaeological record than the ones that can be related to past behavior". Cultural and non-cultural factors represents two sides of the same coin—have we always examined each side with equal care? When the ground rules are carefully set forth and the cultural and non-cultural facets of seed deposition vigilantly explored, there seems to be a possibility that shallow, open sites as well as those over a meter deep may preserve a prehistoric seed fraction that appears unburned.

Regional analyses of subsistence, particularly prominent in the last five years, allow us to better visualize the utilization of cultivated and domesticated crops, the nature of the fields and the encouraged plant pioneers. Probably no editor had a tougher scramble to keep North American papers current than Richard Ford (1985). The emerging evidence for prehistoric cultivation of agave and barley in the west and the domesticated *Chenopodium* and *Amaranthus* in the Eastern United States tremendously complicated the publication of his timely volume. Regional summaries by persons with the knack of distinguishing true subsistence variability in site assemblages from differences induced by variation in methods of data collection, analysis or degree of preservation are needed continuously.

Ethnobotanists have been passive bystanders while hunter gatherer foraging strategy proliferates along with numerous other models for the preconditions for the spread of agriculture. Often models are not stated in such a way that they can be tested with archaeological or ethnobotanical data. The missionary zeal of the theoretically oriented would benefit by struggling with the uncertainties of organic preservation and related realities. Ethnobotanists in turn, could assist more with theory and with its translation into practical, testable hypotheses.

CHARCOAL AND FECAL ANALYSIS

The questions asked in 1963 of wood charcoal analysis (Western 1963:151) resemble those considered in site reports today. What kinds of trees and shrubs have been used for firewood, in construction and in the making of objects for daily and ritual use? Does the analysis of wood types reveal anything in regard to: (1) climatic differences; (2) nature of local vegetation (forest, scrub or grassland); (3) the importation of timber for special purposes; (4) the extent to which the qualities of different woods were appreciated? However, if the question in regards to whether or not cultural selection of woods is practiced fails to receive priority, then all three of the preceding questions prove particularly difficult to answer. If for example only ponderosa pine and fir are used as beams in a site which is today surrounded by pinyon and juniper, are we looking at a change in vegetation or the importation of timbers for structural purposes. If hickory charcoal predominates in hearths, can we conclude hickory was the dominant forest tree? To what extent is cultural selection active in each case?

Only when mild post-excavational curiosity about the charcoal content of a site is replaced by problem oriented provenience sampling can we discard weak interpretations based on often explicit but questionable assumptions. At some sites it would be possible to determine a lack of bias on the part of the inhabitants in choice of wood if the type used for beams is the same as used in a quick hot fire, in the baking pit, and in the smelting furnace. It would also be possible to compare floated charcoal splinters

to larger pieces from the same provenience to investigate differential destruction of charcoal types.

Many ethnobotanists, instead of employing cross sectional views of charcoal as a preliminary step in identification, use it as the only source of evidence. Now if specialists who identify modern wood regard the study of a cross section as only an initial step and secure additional information from radial and tangential sections, I fail to see how those among us who do so much less than that with prehistoric charcoal can hope to achieve a comparable level of reliability. We are dealing with an undetermined range of anatomical patterning in wood created by juvenile twigs as well as some shrubs and trees whose anatomy remains undescribed in texts. Current work could strive to match the standards set over 40 years ago in the identification of fish weir stakes and wattles from Boylston Street, Boston (Bailey and Barghoorn 1942). To recapture this standard requires both proper tools and training.

Using the right tool helps get any job done quickly and well. In 1972 the Forest Product Laboratory recommended a microscope with incident light (vs. transmitted) to examine wood. In today's technology an ordinary dissection microscope of 30x has been replaced by metalurgical microscopes that have magnifications between 50 and 150 power and a working distance of 3 or 4 cm. or better. Such microscopes may already be employed in anthropology departments for lithic wear and pottery temper analysis. In a freshly broken piece of charcoal, critical features like spiral thickening, pitting on individual cell walls, or sieve plate details are apparent with no further preparation. It is true the same job can be accomplished with a Scanning Electron Microscope, but critical details of dicotyledon anatomy can be obtained more efficiently with a metallurgical microscope.

Courses in plant anatomy and the technology of microslide preparation are rarely offered these days, and unless they are offered by someone around long enough to reach the rank of full professor, teachers are apt to be under qualified. Ethnobotanists younger than age 40 have reduced access to formal training in plant anatomy and many are unaware of how beneficial the acquisition of an educated eye and the ability to read the literature with true understanding can be.

From the 1970s onward archaeologists and ethnobotanists have good naturedly accepted studies of the seed and pollen fraction of human feces without questioning the possibility of directly documenting the inevitable greens that are part of omnivorous dining. I know of two exceptions: a thesis from the University of Colorado (Stiger 1977) that deals with coprolites from Mesa Verde National Park and one from Texas A&M. University (Williams-Dean 1978) that concerns archaic coprolites from SW Texas. Throughout the same period the *Journal of Range Management* has regularly carried articles on determining range herbivore diets through fecal and fistula analysis (reviewed by Holechek, *et al.* 1982). Many articles detail problems relating to the quantification of results and their validity, a matter of interest to all. Identification of plant epidermal fragments resembles pollen and phytolith analysis in that it is labor intensive, requires expertise and an extensive plant reference collection.

CHANGES IN PLANT LIFE

Finding a new species as a result of archaeological investigation would be an event equivalent to a paleontologist recovering a new fossil form. In as much as our modern flora has essentially been in place 8,000 years (VanDevender and Spaulding 1979) such a discovery is unlikely. Our nation has been so bombarded by introduced plants that what is truly native can remain unrecognized. At such times the archaeological record makes a genuine contribution to our understanding. The presence of bugseed *(Corispermum)* in hearths, in a Mimbres jar from Swartz ruin, New Mexico and in human coprolites from Cowboy Cave, Utah can be helpful finds when their age has been con-

firmed by accelerator radiocarbon dating (Betancourt 1984). Furthermore, the "useful weed" status of some annuals now rare or locally extinct can only be appreciated from their broad distribution in archaeological contexts (Bohrer 1978). Modern more aggressive introduced plants have altered their role in disturbed habitats.

Local changes in southwestern vegetation have been of such a magnitude that a contrast is apparent within the lifetime of one person. Historic changes in dominant vegetation have been acknowledged recently in what may become a standard reference of the biotic communities of the American Southwest (Brown 1982). Our semi-desert grassland is almost obscured by scrubby trees, brush and cacti. Two native shrubs, burroweed *(Isocoma tenuisecta)* and snakeweed *(Gutierrezia sarothrae)* have replaced grass understory over millions of acres and serve as indicators of former grasslands (Brown 1982:131). In west central Arizona the grassland has shifted from perennials to introduced annuals (Brown 1982:129). In conifer woodland of the Great Basin, junipers have invaded large areas of former grassland (Brown 1982:53). Arizona climax cienegas and marshlands have decreased greatly since 1890 (Hendrickson and Minckley 1984).

The tendency has been to account for modern plant assemblages by considering insufficient limiting factors such as either soil or climate. All too seldom is the biotic factor brought to attention, e.g. in terms of competition from other plants, grazing or human intervention. The grazing industry has vested interests in not recognizing the role of overgrazing in the loss of grassland, just as the lumber industry does not speak of changing the composition of the primieval forest.

Two organizations in the Southwest have evolved different approaches to environmental reconstruction. SARG, the Southwest Anthropological Research Group, discovered one plant ecologist (Küchler 1964) who takes the anthropogenic factor seriously into account when mapping vegetation (Effland 1978). Küchler predicts what a modern plant assemblage would become if suddenly today human influence were removed. Unfortunately for archaeologists, Küchler's potential vegetation maps represent predictions that lack the supposition that landscapes will revert to pre-conquest vegetation. The link to the past is missing. In contrast, environmental reconstruction on the Gila Salt Aqueduct Survey in central Arizona (Fish 1985) began by obtaining numerous (N = 400 +) pollen samples from a variety f sites. The importance of redundant information and duplication of effort became apparent by the internal patterning between suites of pollen samples. A valuable data bank of cultural and ecological information was created. Suzanne Fish could recognize anthropogenic pollen floras because of their characteristic signature in archaeological sites (an abundance of insect pollinated Arizona poppy *Kallstroemia*, spiderling *Boerhaavia*, and globemallow *Sphaeralcea)* contrasted so sharply with pollen spectra from locations away from intense human activity. Modern analogs to the prehistoric disturbance flora proved difficult to locate. Biotic factors active in the creation of a disturbance flora have changed in intensity and direction. Suzanne warns us (1985:84) "If disturbance taxa which disperse relatively little pollen can influence archaeological spectra so greatly, interpretation of environment and climate from more prolific producers should proceed with great caution." Our limitations are increasingly apparent.

No neat, easy formula helps one detour around the dangers in the reconstruction of vegetation. The uncertainty (nay, disagreement) in attributing vegetational change to cultural or natural causes in the archaeological record has been pointed out recently by several authors (Bryant and Holloway 1983; King and Graham 1981). Overly confident attempts to quantify past food resources based on the presumed distribution of vegetation, carrying capacity and calories have been numerous in recent years (see Moran 1984). A more conservative view of environmental reconstruction is expressed by King and Graham (1981:139) . . . "it should be possible to develop the knowledge of a few key taxa necessary to fairly accurately quantify those resources and to project variations

in their relative abundance and distributions back through time as they may have responded to environmental change."

CONTEMPORARY STUDIES

It is never too late to conduct contemporary ethnobiological studies. I think many modern studies are better than years ago because of the ecological orientation. Even though previous studies existed on southern California Cahuilla ethnobotany, the work of Bean and Saubel (1972) provided important insights into the manipulation of plant life. One can seek examples from other parts of the world, but those nearest one's own territory seem to carry more impact. For example, when I read how fan palms are planted from oasis to oasis by the Cahuilla, it was easier to understand how agaves might have found their place on terraced slopes in southern Arizona (Fish *et al.* 1985).

No matter what our task is in ethnobotany, no matter how supposedly tedious, it needs to be treated as important and worthy of our best efforts. The seemingly insignificant record that Gary Nabhan made of Arizona barley grass in Papago fields took on a new dimension when viewed from the perspective of a palynolgist working with the prehistoric pollen rain. Research by David Clawsen (1985) inadvertently helped us understand the color coding of maize that corresponds to the symbolic colors of the sacred directions among the pueblos. Although ritual observances frequently reinforce practices (once) vital to the economic survival of a people, the particular advantage bestowed by preservation of strains of colored maize has been obscure. David Clawsen tells us color coding of crops such as potatoes, yams and many legumes helps a cultivator plant according to given maturity periods. The farmer who maintains a greater diversity of colored varieties of any one crop carries a kind of physiological crop insurance. Despite how the rains distribute themselves, or whether or not the frost is early or late, the chances are good that there will be some crop to harvest. Clawsen has provided ethnobotanists working with the archaeological record a lost dimension, for by the time we examine the physical remains of any crop, all the varieties are the same color, *black*.

ETHNOBOTANISTS AND ETHNOBOTANICAL INTERPRETATION

Ethnobotanical interpretation of prehistoric plant remains should consider factors like the method of excavation, sampling design, context of recovery, and post-depositional factors until all pieces of the puzzle fit in harmony (Fig. 1). But there are aspects of interpretation that transcend site specific boundaries. One must provide perspective derived from a broad range of experience in dealing with the archaeological plant record (directly or via literature) and from the rich, deep kaleidescopic patterns formed by modern and historic interrelationships of plants and man. Archaeological botanical evidence can be likened to a crumb surviving a whole loaf of bread. Or envision it as a stanza of a longer ballad. One needs to provide the refrain that brings unity, lest such botanical stanzas land in the scrap heap of our intellect. A supra-tribal, supra-national viewpoint combined with a competent ecological perspective can supply vitality to any otherwise drab scholarly discussion. Am I asking too much? The way our society organizes itself makes preparation for the task more difficult.

The compartmentalization of knowledge into fixed disciplines works against the creation of an ethnobotanist. The time needed to acquire a dual background often proves excessive unless one begins as a undergraduate. It is none-the-less essential to develop the interdisciplinary vocabulary and theoretical understanding that allows one to read the literature directly, for therein lies a rich reservoir of ideas and a bridge to alleviate misunderstanding.

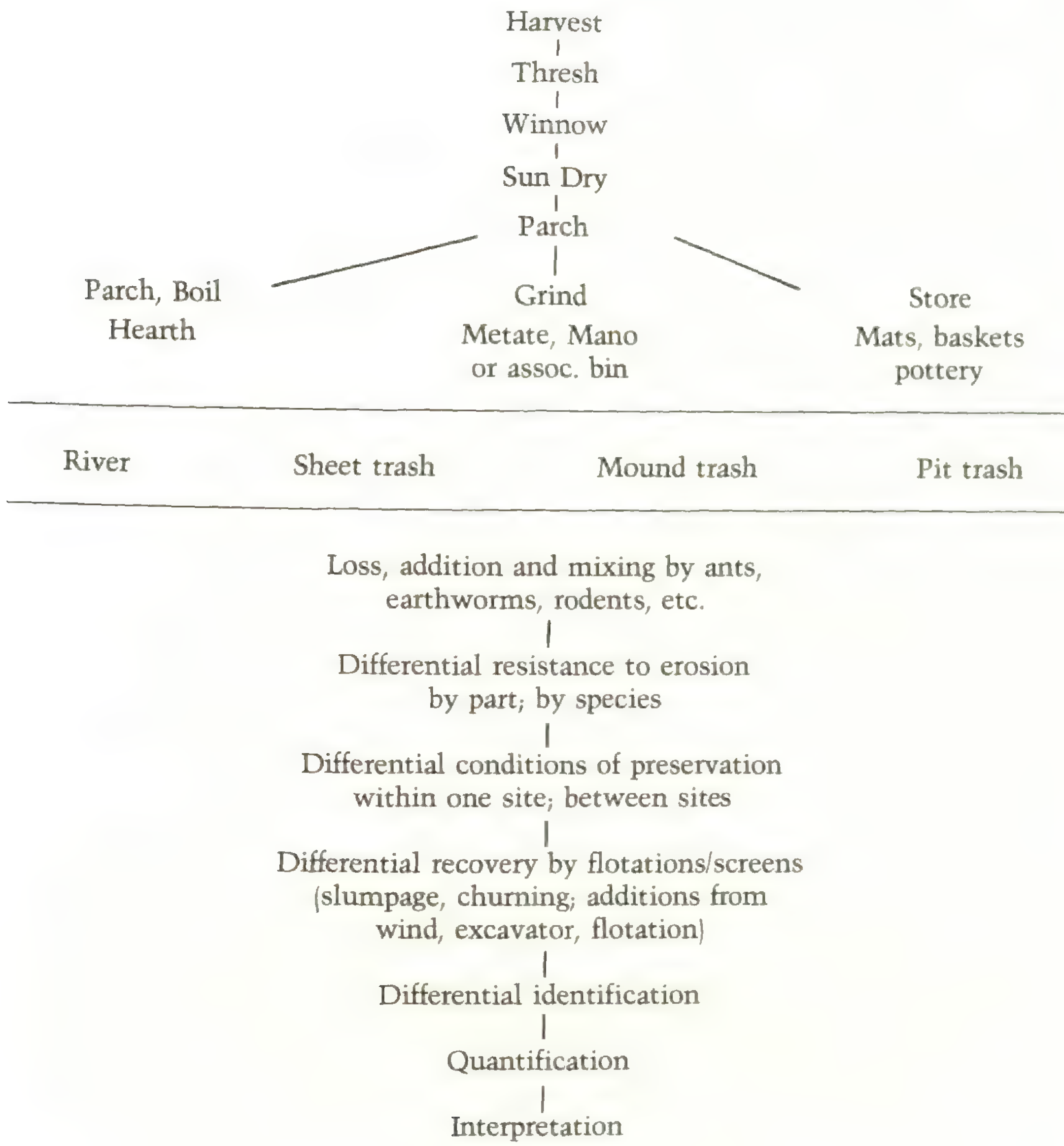

FIG. 1.—The formation of the archaeological seed record.

Neophyte ethnobotanists greatly benefit by being shown how the abstractions of the textbook apply to the field. In subjects like plant anatomy, taxonomy, ecology, evolution and core courses in anthropology, content is commonly emphasized over developing skills in problem solving. To resolve a baffling question a more experienced researcher can supply the impetus for a younger colleague to try a new approach or re-evaluate an old one. Many practicing ethnobotanists have incorporated these skills (Toffler 1971:414) in their own approach to problem solving: (1) classifying and reclassifying information; (2) determining its veracity; (3) shifting categories when necessary; (4) moving from the concrete to the abstract and back again; (5) looking at a problem from a new direction; (6) shifting from the inductive to deductive methods and back again.

As ethnobotanists exist in increasing numbers as independent consultants, a form of ancillary or off-campus research internship would seem desirable for students. If problem solving skills are not formally taught, opportunities should be available where

skills can be developed. In addition, the research topics that intrigue professionals who are directly and heavily involved in research are apt to be more original than well-massaged topics like the origin of agriculture.

In the past people with ethnobotanical interests have found positions in colleges, universities or museums. Formerly men with doctorates held academic positions exclusively whereas now they are joined by a small group of women. Traditionally (since the 1930s) the museum has nourished and protected ethnobotanical careers of men and women of all levels of education. I am reminded of the support of Melvin R. Gilmore and Volney H. Jones at the Museum of Anthropology at the University of Michigan, of Margaret Towle's association with the Harvard Botanical Museum, of Albert Whiting with the Museum of Northern Arizona, of Hans Helbaek and the National Museum of Copenhagen, Jaques Barrau at the Museum National de Histore Naturelle in Paris, Edgar Anderson and Hugh Cutler of the Missouri Botanical Garden. The trend continues to expand as many additional museums (such as the Bernice P. Bishop Museum in Honolulu and the Arizona State Museum in Tucson) find places for people with ethnobotanical skills.

RESERVOIRS OF IDEAS AND INSPIRATION

Everyone, including the ethnobotanist to be, must ask, "What shall I read?" "What is worth reading again and again?" "What are the classics?" Sometimes we read to know the level of understanding achieved in times past, but the real value is in capturing "the visions and the styles of the scientists who preceded us" (Thompson 1984:187). Short articles seldom contain such an essence and short articles indeed predominate the ethnobotanical literature. Really good autobiographies or biographies of ethnobotanists are all too difficult to find: their lack impoverishes us. My own library contains some favorite books: Edgar Anderson's (1954) *Plants, Man, and Life*, Frank Cushing's (1920) *Zuni Breadstuff*, M.P. Harrington's (1932) *Tobacco Among the Karuk*, W.W. Hill's (1938) *The Agricultural and Hunting Methods of the Navajo Indians* and Paul Weatherwaxe's (1954) *Indian Corn in Old America*. More recently I have added Gary Nabhan's (1985) *Gathering the Desert*.

The above list contains no volumes, chapters or appendices in archaeological site reports because so few are models of "how to know, how to think, and how to write" (Thomson 1984:187). Few contain the flavor of imaginative thought, clean, stylish prose and front-line science (Thomson 1984). But there are some. I have already discussed one recent example (Toll 1984) under flotation. Toll has distilled voluminous observations into a single paper with breathtaking conciseness. The facts at her disposal do not overwhelm her. Another example is Goodyear's (1975) Hecla II and III. Goodyear is an archaeologist that sees no barriers between anthropology and botany. The waters of each intermingle and sustain him as he develops detailed methods of sampling cultural and botanical data and constructs models of prehistoric subsistence activity in remote areas of southwestern Arizona. The strength and weaknesses of the techniques employed are candidly presented while the author places his work in a broad regional perspective. His manner of investigation has been rightfully described as thorough, creative and efficient (Gumerman 1977:12).

Several examples of how to know, think and write date from the 1950s. One of these is V.H. Jones and R.L. Fonner's Appendix C *Plant Materials from Sites in the Durango and La Plata Areas Colorado*. Every shred of information is given the courtesy of thorough treatment and the line of reasoning clearly presented. Findings are placed against a background of knowledge of plant utilization so carefully documented that later finds of cultivated amaranth are anticipated. The discussion of maize is unusually complete and lucid. The long period of evolution of the published report is traced for the reader.

The other example is Hans Helbaek's (1952) *Early Crops in Southern England.* His understanding of what constitutes crops is truly refreshing, for he includes far more than wheat and barley. Flax, rye and crabapples; chess and oats weave in and out of discussions. The role of the *Brassicas, Chenopodium, Polygonum* and *Galeopsis* as plants of cultivated fields and gathered food sources forms a balanced part of his concern for early crops. His tables tell of the increase in grain size with the continued cultivation of several crops and one learns of the waxing and waning of the prevalence of naked barley against the backdrop of other available choices, including rye. Never is southern England treated as an island whose plant history is devoid of connections. The context is European and the scope epic in its coverage of the neolithic, bronze and early iron ages. The keeness of his observation and his critical perception of events prevails in his writing. Would that we could achieve as much in the years ahead.

REFLECTIONS

The history of research on maize is one of looping back over territory once thought to be conquered. Cob and kernel measurements, pollen identification, phytolith recognition, carbon 14 dating and corn evolution—all have been the focus of controversy. Has maize been such a popular band wagon that some people have published prematurely? Or is it that newly developed techniques to deal with maize have been readily accepted without critical evaluation? As we gain enthusiasm for the human capacity to begin to tinker with the evolution of many other plants besides maize, can not this same thing happen with every new species proposed as a cultivated or domesticated plant? The above described trends in the study of maize are ones that I would rather not project into the future. Researchers who worked with ethnographic maize and those who dealt with prehistoric remains sometimes hummed little tunes the other did not hear. In similar cases the meetings of the Society for Ethnobiology can serve as a forum.

It is the larger archaeological projects that seem to have made the most headway with methodological problems concerned with flotation and pollen analysis. This is troubling, for larger projects are dwindling and the burden should be shared more evenly. Enough site specific studies have accumulated that syntheses are being produced, an admirable trend that should continue. Presently we perceive common denominators in subsistence practices without daring to ask "Why?" A greater participation in the theoretical realm of model building and model demolition surely must be near.

Attempts to reconstruct vegetation have varied in approach and results. We have had to learn a lot about our limitations. We may have to be content with estimating abundance of a few species rather than dealing comprehensively with absolute abundance. By analyzing a spectrum of contexts in which prehistoric plants occur can we discover something of their former competitive relationships? The disturbed ground plants of today and of pre-contact times were not always one and the same. The role of soil and climate in influencing the formation of a given plant community has enjoyed exclusive popularity too long. The biotic factor, including the anthropogenic one, also needs careful consideration. Nevertheless, the path is still not all that clear. Our growing humility might be transmitted to archaeologists who still believe in miracles.

Museums have served and probably will continue to serve as a focal point for ethnobotany. Apprenticeships to museums and other independent organizations active in sponsoring research may be the best way for a prospective student to develop skills and apply book learning to concrete issues. It is never too late to conduct contemporary ethnobotanical studies. Field work in the ethnographic and ethnobotanical realm coupled with omnivorous reading sharpens appreciation for the multifaceted patterning of plant-man interrelationships. While it still can be secured, formal training in plant anatomy can reverse a downward trend in the quality of reports concerning charcoal,

coprolite content and related applications. If ethnobotanists would first dream that archaeobotanical site reports might serve as reservoirs of ideas and inspiration, it would not be long before we could read them. A viewpoint that transcends both tribe and nation blended with a competent ecological perspective can, with continual grooming, guide others to "New Directions in Ethnobiology".

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Prehistoric Food Production in North America. Richard Ford, editor. Museum of Anthropology, University of Michigan Anthropology Papers, No. 75. Ann Arbor: University of Michigan Press, 1985.

This volume of eleven papers assesses plant domestication, diffusion, horticulture, and the effects of these processes on the prehistoric human cultures north of Mexico. Originally derived from presentations made at an advanced seminar at the School of American Research in March, 1980, these manuscripts reflect a fertile interaction between selected archaeologists, archaeobotanists and crop evolutionists. Both its successes and failures are consequences of its transition into book form during a watershed in American ethnobotany.

First, the outline of the structure of the book and its unifying themes. In an excellent preface by Richard Ford, written four years after the seminar on "The Origins of Plant Husbandry in North America," the following issues are outlined as the themes which participants were asked to address. The authors who contributed most to discussion of a particular issue are noted in parentheses.

1. Demography and subsistence patterns at the time of crop introduction from Mexico (Patty Jo Watson on Mississippi watershed cultural changes; Paul Minnis on archaic economies in the Southwest).

2. Modes and routes of crop diffusion (Dee Ann Story on the proposed east Texas route via the Gulf Coastal Plain; Walton Galinat on eight-rowed maize diffusion; Frances King on cucurbits).

3. Genetic characteristics of early crop introductions (Charles Heiser and Frances King on cucurbits; Galinat on maize; Asch, Asch and Heiser reviewing chenopods, bottlegourds and tobacco, and Ford's summary).

4-5. Integration into existing economies, and resulting changes (Minnis on the Southwest, Watson on the Mississippi Valley; and Ford on regional crop complexes).

6-7. Technological intensification of food production and constraints to intensification and expansion (Ford and Minnis on processing technologies; Watson on material correlates of agriculture; and Cowan and Minnis on environmental management through burning, irrigation and transplants).

8. Effects on sedentarism (Watson and Minnis on respective regions).

9-10. Plant domestication, breeding or varietal evolution north of Mexico, and its recognition in the archaeological record (Heiser on sunflowers, sumpweed, and minor cultigens; Cowan on the same, as well as on trees; Asch and Asch on knotweed, maygrass and little barley; and Galinat, Minnis and Berry on changes in maize; Berry and Minnis on early dates of domesticates).

These annotations suggest that there was quite a bit of overlap in coverage of certain items, such as squash, sunflower, maize and chenpod prehistory, and difficulties with dating early maize and cucurbits. While this redundancy may make some readers skip over reiterated details from one paper to the next, the authors must nevertheless be commended for tackling common problems from different perspectives. In this manner, we learn that acceptance of sumpweed, giant ragweed, devil's claw, jerusalem artichokes, knotweed, groundnuts and panicgrass as true domesticates was open to debate at the time of the seminar. Early introduction dates for *Cucurbita pepo*, *Lagenaria siceraria* and more conservative dates for *Zea mays* were being accepted. The Eastern cultivated chenopod was being more confidently identified as *Chenopodium berlandieri* var. *nuttalliae*, and *Cucurbita pepo* var. *texana* as a wild progenitor or as a feral (regressive) form was being seriously reconsidered.

Most importantly, this book, and in particular, Richard Ford's synthetic essays, allow us to better compare and contrast the development of horticulture in the central

Mississippi watershed and the U.S. Southwest. Although the book does not truly cover North America—it excludes the Upper Missouri, Florida, the eastern seaboard, the Great Basin, the western Plains, and northern Mexico—it is a valid advance toward a continental synthesis on horticultural evolution. Since North America (north of Mesoamerica) is still regarded by Europeans, the USDA and many Latin American ethnobotanists as altogether lacking an agricultural history which includes indigenous domestication and diversification, this volume proves that North Americans need not travel to other continents in order to study crop evolution.

Yet Richard Ford's Preface admits the volume's unfortunate flaw—the years following the conference witnessed "the most productive period in the history of American ethnobotany". Ford notes that "Numerous cultural resource management projects have yielded an unprecendented quantity of plant remains from archaeological sites throughout the United States". The efforts to integrate these results into various chapters in the four years of manuscript revision prior to publication were uneven, and generally inadequate, since subsequent advances radically changed the picture presented at the 1980 conference. By the time the volume was distributed, new dissertations, journal articles and books had clarified the status of chenopods (Wilson), barley (Adams, Bohrer and Gasser), agaves (Miksicek, Fish and Bohrer), pepos (Wilson and Conard), devil's claw (Bretting, Nabhan, Whiting, Dobyns and Euler), panicgrass (deWet, Kaemlein and Nabhan), mustards (Bye), amaranths (Miksicek), and saltgrass (Felger and Yensen) and numerous "wild" plants in the East (Munsun, 1973). Maize diffusion and classification had been rewritten by Doebley and Goodman, and Merrick had reported on new insights into *Cucurbita mixta* and *C. moschata* at national meetings. Miksicek, Winter, Bohrer, Gasser and others have found earlier dates of crop introductions (tobacco, amaranth, etc.) into the Great Basin and Southwest. More disconcerting, turkey prehistory has lately been rewritten by Rea, McKusick and Steadman, but animal domesticates are hardly noted in the volume at all. Zooarchaeology by Szuter, Bayham, White, Emslie, and Fritz fails to merit any mention in the transition from gathering and hunting to agriculture *plus* hunting and gathering. New hypotheses on the origins of agriculture by Rindos (1984) and Johns supercede many of the ones presented here, although they remain controversial.

There is no fault in the editor or authors for having some of their interpretations become obsolete due to new advances. That portions of this well-written book are already outmoded signifies great growth in interdisciplinary efforts, to which these scientists have all contributed. This obsolescence was not planned, nor was the seeming exclusion of other work. In short, *Prehistoric Food Production* offers a snapshot of a landscape that has already changed since this book's conception. All North American ethnobiologists are indebted to those included in this volume who have laid such a groundwork for advancing a synthetic understanding of American agricultural origins.

Selected References:

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Rindos, David. 1984. The Origins of Agriculture: An Evolutionary Perspective. Academic Press, Orlando.

For other notes on recent developments in North American plant domestication, readers are referred to articles which have appeared in *Journal of Ethnobiology*, *Economic Botany*, *American Journal of Botany*, *American Antiquity* and *Desert Plants*.

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Of Plants and People. C. B. Heiser, Jr. University of Oklahoma Press, Norman, Oklahoma. 1985. Pp. xiii, 237; 65 figs.

Professor Heiser, who has published several books on economic plants of the Andean region, has a facility in sharing his enthusiasm and interdisciplinary outlook for the study of many of the poorly known economic plants of the Andes with his readers. An accomplished and experienced field botanist, his writings encompass botany, anthropology and sociology and bring out the often major roles that these usually minor crop plants play in the life of native peoples in South America.

This new contribution is, in the words of the author, "unlike my other works . . . not confined to one subject or to a single group of plants . . . and all of the plants have interesting interactions with people." In addition to its value to botanists and anthropologists, he has successfully striven to "write for the student and amateur in mind."

The book is divided into 13 chapters: Pepos and people; Totora and Thor; Little Orange of Quito; Chenopods; Sangorache *(Amaranthus)*; Trip to Tulcan; Lupines; Green "Tomates" *(Physalis)* and purple "Cucumbers" *(Solanum)*; Peppers, Peperomias; Sumpweed *(Iva)*; a Plague of locusts, Seeds, sex and sacrifice—religion and the origin of agriculture. The extensive bibliography is divided into sections, each chapter with its own list of items and often analytic notes.

It is to be hoped that the appearance of this type of book may encourage the publication of others in the field of ethnobiology. Ethnobotanists will want to have **Of Plants and People** at hand on their shelves.

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NEW DIRECTIONS OF PALYNOLOGY IN ETHNOBIOLOGY

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ABSTRACT.—Archaeological palynology, fairly new as a discipline, originated in studies conducted less than 50 years ago. During this early developmental stage, it suffered because of: (1) inadequate communication between botanically-oriented palynologists and field archaeologists, (2) differences in conceptual orientation between archaeologists and archaeological palynologists, (3) fossil pollen studies of archaeological sites often had been incorporated into final reports only as appendices, (4) a growing number of studies on the fossil pollen remains of archaeological sites were published almost exclusively in "contract-type" reports where the number of printed copies was limited and distribution is restricted, and (5) sophisticated, statistical techniques had not been applied to fossil pollen data from archaeological sites. Our article, after a brief history of archaeological palynology, discusses some of the applications of fossil pollen data in archaeology, and offers suggestions and concerns about the course of the discipline in the years to come.

INTRODUCTION

Palynology, broadly defined as the study of pollen and spores from both living and fossil seed plants (Hyde and Williams 1944), includes research in such various areas as pollen production and dispersal, composition and morphology of the exine (outer pollen wall), applications in stratigraphy and paleoecology, preservation, and in explaining man-plant interactions. Even though the field has a history of over 70 years, only during the past several decades has significant emphasis been placed on analysis of fossil pollen from archaeological materials. Although most archaeologists know of the basics of palynology, many of them are still unaware of the full range of data which palynology can provide.

As a discipline, palynology is relatively young and its applications to archaeology are still being developed and refined. Thus, we feel that the full comprehension and appreciation of the impact and importance of archaeological palynology, requires a review of its historical development, an examination of its data base and specific techniques, a survey of the range of research questions which are currently arising, and finally, an exploration of the potentials of this discipline for future research needs in archaeology.

HISTORICAL REVIEW

Pollen analysis, initially defined as the calculation of pollen percentages, appears first to have been attempted on Swedish peat deposits by Gustav Lagerheim (in Witte 1905). In 1916, at a convention of Scandinavian naturalists (Davis and Faegri 1967), von

Post explained the potential of this technique by presenting the first percentage calculations of fossil pollen. As Faegri (1981) observed, von Post regarded pollen analysis primarily as a method for dating Quaternary-aged sediments from northern Europe, although later he acknowledged the technique also could be a useful tool in paleoecology. The initial development of pollen analysis can therefore be viewed as a continuation or extension of the original palaeophysiognomic school which was exemplified by Blytt (1876). According to Faegri (1981:45) von Post's pollen analysis was, "pure palaeofloristics: the registration of the occurrence and (what was new) relative frequency of taxa . . .". The results and interpretation of von Post's analyses were not based on pollen analysis *per se* but on the techniques and data base associated with the palaeophysiognomic school (Faegri 1981).

The influences of the palaeophysiognomic school can be clearly seen in the construction of the early pollen diagrams. The palaeophysiognomic school accentuated its emphasis upon the arboreal (tree) component of the forests. Thus, pollen from plants confined to the understory levels in the environment (non-arboreal plants) often were ignored or were calculated only as a function of arboreal pollen. Unfortunately, this limited approach to the study of fossil pollen became the standard during the early part of the twentieth century.

During the early 1900s, the field of pollen analysis did not produce the types of comprehensive fossil pollen studies which would become standard in later years. This problem was created initially by three main factors: (1) a poor understanding of pollen morphology; (2) the use, not the lack of availability, of poor quality optical equipment and (3) the use of inadequate laboratory extraction techniques for separating fossil pollen from matrix materials such as peat, soil, sand, and coprolites (Faegri 1981).

The concept of using an ecological approach in pollen analysis was first introduced by Auer (1927) and later by Sears (1930a, 1930b) in their studies of North American sediments. In Europe, similar concepts were first introduced at the 1933 meeting of the Baltic Committee (Faegri 1981).

Once the ecological approach was introduced as a proper avenue of investigation, the directions and potential benefits derived from the field of pollen analysis were changed forever. However, in spite of the demonstration of the new ecological approach to solve problems in fossil pollen studies, the influence of the older, synecological (palaeophysiognomic) school, was still evident in North American fossil pollen analyses up through the 1950s. This is evidenced by the reliance of palynologists such as Hansen and Potzger, upon arboreal pollen counts as the primary basis for their interpretations and constructing of pollen diagrams. Thus, as in the initial studies by von Post and others, these later North American palynologists tended to consider non-arboreal fossil pollen counts as representing unimportant information in the interpretation of paleoenvironmental data.

Almost immediately after the acceptance of an ecological perspective emphasizing ecological questions, palynology expanded to incorporate information related to the influence that man may have had upon prehistoric plant communities. In what seems to be the first pollen analysis of an archaeological site, Jessen (1935) reported on the fossil pollen analysis of sediments in North Jutland, Denmark. Later, Iversen (1941) again demonstrated the importance of using fossil pollen data in archaeology when he successfully dated the beginning of the Neolithic period in Denmark. He based his findings on the decline of local *Ulmus* (elm) pollen in sediments, which showed the appearance of herbs and weedy plant pollen normally associated with human disturbance. The first appearance of agriculture in southern Denmark was again documented by the later fossil pollen studies of Faegri (1944).

Iversen's and Faegri's studies not only defined with fair precision the first arrival of agriculture into northern Europe and evidence that cereals of Middle Eastern origin

were planted, but they also noted how cultures had altered the environment by clearing the forested areas using slash and burn techniques. Some of the later fossil pollen studies related to origins of agriculture in other regions of the world include Donner (1962), Durno (1965), Godwin (1944a, 1944b, and 1956), Iversen (1949), Mitchell (1951, 1956), and Morrison (1959).

Following Iversen's (1944) example, other palynologists began using fossil pollen to clarify and interpret archaeological data. Troels-Smith (1960) examined pollen and seed remains from the Muldbjerg archaeological site located in West Zealand, Denmark. Based on pollen and plant macrofossil analysis of deposits from that Neolithic-age site, he reconstructed aspects of the past environment and showed how the overgrazing of domestic animals in that region of Denmark had created dramatic changes in the local plant composition. Dimbleby (1960) showed how the fossil pollen record from a Mesolithic site in England demonstrated changes in prehistoric forest composition caused by early man's shift from a hunting and gathering economy to one which focused upon agriculture and pastoralism. Dimbleby (1963) conducted another fossil pollen study near Addington, England where he introduced a technique for calculating the concentration of fossil pollen in soils. He then used those pollen concentration values to show how Mesolithic cultures had modified the local vegetation.

Applications of pollen analysis in North American archaeological sites was also progressing. Paul B. Sears was one of the first researchers in North America to apply pollen analytical techniques to solving archaeological problems. In an early article, Sears (1932) suggested that a possible cause for the Hopewell expansion into areas of the eastern United States may have been a climatic shift that favored the growth of maize. Later, Sears (1937) conducted some of the pioneering pollen investigations of archaeological and non-archaeological sediments in the American Southwest. Although Sears' initial attempts in the Southwest (Sears 1937) resulted in only limited success, he and others (Sears and Clisby 1952; Clisby and Sears 1956) continued examining open sites in the American Southwest while Roger Anderson (1955) began trying to recover fossil pollen from cave sediments. The work of these early palynologists demonstrated that fossil pollen was often difficult to recover but that it was present in many Southwestern sediments. In the late 1950s Paul S. Martin and his graduate students began a detailed look at the fossil pollen in lakes, peat bogs, and archaeological sites in the Southwest. Their studies convinced others that even though some deposits in the Southwestern United States were barren of fossil pollen, other areas yielded good records (Martin 1963).

The next two decades saw new developments in archaeological palynology. First, during the 1960s fossil pollen studies of archaeological sites became more common (Cox and Lewis 1965; Godwin 1967; Tsukada and Deevey 1967; van Ziest 1967); and secondly, the 1970s saw a dramatic rise in contract archaeology. The development of contract archaeology programs throughout the United States was caused by increased federal funding and resulted in a large quantity of archaeological fossil pollen studies, especially in the American Southwest. Unfortunately, most of these reports (both palynological and archaeological) were never published, and the data recovered lie buried in the offices of many federal and state agencies. Since there is no central clearing house for palynological contract reports, for all practical purposes, the majority of these reports are not available as reference sources in most major libraries. Hall (1985) has provided some help by compiling a bibliography of southwestern palynology which includes many of the contract studies.

VALIDITY OF POLLEN ANALYSIS

Pollen analysis is a useful research technique in paleoenvironmental reconstruction and archaeology because of two important criteria: (1) preservation and (2) recognition.

Most pollen grains and spores produced by terrestrial plants have a chemically stable outer wall, called the exine which preserves well in many sediments because it is composed of a mixture of cellulose and a more durable compound called sporopollenin. Sporopollenin is composed of oxidative co-polymers of carotenoid and carotenoid esters (Shaw 1971) which are highly resistant to decay and have enabled ancient pollen and spores to remain preserved for millions of years. In addition to its preservation quality, extensive research has demonstrated that pollen grain characteristics and wall morphology are consistent within a plant species, but vary from the pollen morphology of other plant taxa. These differences permit the pollen of one genus, or species, to be recognized from types produced by other plant taxa.

The utility of pollen analysis is based upon two major assumptions: (1) any particular fossil pollen assemblage is indicative of the original pollen contributing flora; and (2) the recontructed fossil vegetational communities can be used to infer paleoecologic and paleoclimatic parameters. The first assumption appears warranted subject to certain limitations. Plants produce varying amounts of pollen or spores which are dispersed and are carried by wind currents (anemophilous), water currents (hydrophyllous), or by different types of animals (zoophilous). Several researchers (Andersen, 1967; Erdtman, 1969) have estimated relative pollen productivity rates and attempted to correlate these with the mode of pollination, i.e. anemophily, etc.

After being dispersed, pollen falls to the earth's surface in the form of a pollen rain which is influenced by a number of factors such as the rate of fall (Wright 1953); the method of transport (Janssen 1970); the effects of atmospheric and climatic conditions (Tauber 1965, 1967); and the effects of pollen recruitment into lake-type sediments (Bonny and Allen 1985; Davis *et al.* 1985). Once deposited, pollen and spores are subject to various types of degregation which selectively destroy certain pollen types and leave other types uneffected (Bryant and Holloway 1983).

Numerous studies have also been conducted which address the presumed relationship between the recovered fossil pollen assemblage and the plant community from which it was derived (King and Kapp 1964; Andrews 1967; O'Sullivan 1973; Adam and Mehringer 1975; Birks *et al.* 1975; Webb and McAndrews 1976; Webb *et al.* 1981; Heide and Bradshaw 1982; Holloway 1984). Recent numerical techniques, which are presented in a later section, have been very successful in elucidating this relationship. The successful application of these techniques notwithstanding, the lack of discrimination of exine morphological characters which would permit taxonomic identification of these pollen grains to the species level, has been a major obstacle in defining the precise relationship between the paleovegetational community and the fossil pollen assemblage.

The second major assumption used by palynologists involves the interpretation of the accumulated pollen data. In each study, the ultimate goal is to infer paleoecologic and ultimately, paleoclimatic conditions; yet to do this we just assume a valid relationship between the reconstructed vegetational community and the paleoenvironment. Both biotic and abiotic factors influence the distribution of certain plants. These pollen and spore producing plants are restricted (today) to specific ecological habitats in which they survive, and in their optimum environments, are most abundant. Drawing on modern ecological studies as analogues to paleovegetational communities, enables the palynologist to effectively test this second assumption.

From the information gained by modern ecological studies, the palynologist can begin to assign ecological parameters to the paleovegetational communities based on the presence of a few characteristic pollen types. This "indicator species" approach (Birks and Birks 1982) is usually successful enough to infer paleoenvironmental conditions. This procedure cannot rely solely on the modern ecological data as many other factors are involved in determining plant biogeographical distributions. As an alternative to the

indicator species approach, Conolly (1961) stressed the importance of including all taxa present, rather than a few "indicator species", prior to interpreting the pollen assemblages paleoecologically. This procedure may be cumbersome, especially with long temporal records consisting of many levels, but the numerical treatment of these data in terms of recognizing co-occurring and co-varying groups of taxa may provide the necessary information to demonstrate the validity of the relationship between the paleovegetational community and the paleoenvironment.

DISCUSSION

During the past several decades, pollen analysis has become a standard procedure during many archaeological excavations. The major problem remaining is that much of the palynological and botanical data has not been completely synthesized with the archaeological data for the purpose of interpretation. Most often, palynological and other types of botanical data are included at the end of an archaeological report in appendix form. There may be an historical precedent for this, yet we suspect that a major cause may lie in the philosophical development of the two disciplines.

Quaternary palynology, as an historically based descriptive science, has a long history of strict inductionist thought. Palynology, as an empirical science depends upon inductive reasoning which proceeds from one observation to the next in an attempt to provide generalities about the environment (Birks and Birks 1981). Edwards (1983) has suggested that this philosophical position may not be intellectually stimulating for some researchers. Although this may, or may not, be the case, the history and tradition of inductive reasoning in this field is strongly entrenched due, in large part, to the influence of historical geology on the field of palynology.

Archaeological palynology is not only tied to both the historically based geological precepts of biostratigraphy and environmental reconstruction, but also to those concepts inherent to the fields of anthropology and archaeology. During the past two decades, much of the emphasis in archaeology has been away from the descriptive techniques of the earlier "culture history" approach (Flannery 1968). thus, the "new" archaeology, which emerged in the late 1960s, concentrated upon analysis, synthesis, and hypothesis testing, emphasizing instead of descriptive work, a processual approach (Binford 1971, 1977; Watson *et al.* 1971; Morgan 1973; Schiffer 1981). This, however, was by no means a universal shift in theoretical orientation, for much archaeological activity during this period was directed, as in palynology, toward data collection using primarily inductive approaches. But we do see among many archaeologists a shift towards incorporation of deductive principles within the archaeologists' research design. As Binford (1983) notes, this does not imply the rejection of an inductive approach, but rather the integration of alternative research strategies in order to arrive at explanation.

We see current archaeology as moving in a direction of scientifically testing explicitly stated hypotheses. Most palynologists, on the other hand, have not yet made this shift and thus continue in their collection of new data (Edwards 1983). This aspect, however, may not be all that unfortunate since many geographical areas are still lacking an adequate, comprehensive data base. For example, much of Texas (Bryant and Holloway 1985), California (Adams 1985), and the Great Plains (Baker and Waln 1985) are known palynologically only from a limited number of sites which in many cases lack adequate geological chronologies.

A secondary problem in archaeological palynology has stemmed from the development of contract archaeology during the last several decades. Often there is too little time alloted for botanical analysis and interpretation, yet many federal and state regulations proscribe paleoenvironmental testing in the initial scope of work. Thus, because of time constraints, the vegetational and environmental data often are not effectively

incorporated into the archaeological analysis. With careful planning, however, this need not be the case. The paleoenvironmental data provided by palynology, even when presented in appendix form, can be incorporated within the interpretation of the archaeological data as in the case along the Yazoo River drainage system in central Mississippi (Thorne and Curry 1983). More of this type of integration is desperately needed in the field of archaeology.

As a first step toward integration between palynological and archaeological data, palynologists need to utilize a rigorous scientific approach. As Edwards (1983) observed, only one paper (Garbett 1981) published in six major journals during 1981, dealt with testing an explicitly stated hypothesis and then discussed the level at which the hypothesis would be rejected. Although in many cases, the acquisition of new palynological data is necessary, in many geographical regions such as the Northeastern U.S. (Gaudreau and Webb 1985), the Great Lakes Region (Holloway and Bryant 1985), the Southeastern U.S. (Delcourt and Delcourt 1985), and the American Southwest (Hall 1985a, 1985b) palynological data are available to afford researchers the opportunity to test models and hypotheses of climatic and vegetational dynamics (Delcourt and Delcourt 1984). It is in this direction that palynology will have its most beneficial effect upon the understanding of the human impact on vegetation and the full utilization of the botanical and archaeological data. Without this, palynologically data will be forever relegated to the appendix.

ANTHROPOGENIC STUDIES

Human cultures modify the natural environment in which they live and thus often alter the ecological balance of the plant taxa which, in turn, are reflected in the local pollen rain. Learning to recognize ancient man's alterations of the environment through analysis of the fossil pollen record is one of the important goals of the discipline of palynology.

There are a number of studies which have focused upon anthropogenic factors as reflected by pollen analysis. Dyakowska (1958) was able to correlate two decreases of forest tree pollen in the fossil records of Poland to known historic events. Studies of cultivated plants, on the other hand, are often the best indicators of human plant modification (Behre 1981) and have generated the majority of archaeological interest (Martin 1963; Hill and Hevly 1968; Leroi-Gourhan 1969; Lytle-Webb 1978). One of the biggest problems with these types of studies has been the lack of adequate pollen morphological characters with which to separate the cultivated from the wild forms of the plant (Behre 1981). Much effort has been spent on the analysis and identification of these crop plant remains at archaeological sites and these studies demonstrate that the best evidence of prehistoric cultivation of plants still comes from plant macrofossils rather than from the fossil pollen record.

The pollen of some non-cultivated plant species are often good indicators of disturbance and thus, by extension, useful in interpreting anthropogenic plant use. For example, Burrichter (1969) recognized the importance of *Plantago* sp. as an indication of rotational cultivation systems and the historic increases in pollen of *Ambrosia* and other weedy species following the advent of European agriculture, especially in the Upper Midwest U.S.A., are well documented (Birks *et al.* 1976; Webb *et al.* 1983; Holloway and Bryant 1985). In each of these cases, identification and interpretation of anthropogenic factors relies heavily upon the ecological and edaphic parameters of the plant species in question (Behre 1981). Because of the differences between prehistoric and modern agricultural practices, modern analogues of pollen recovered from agricultural fields are not necessarily applicable to these types of studies.

Additionally, pollen analysis is useful in the investigation of economic land use criteria. Edwards (1982) has discussed several methodologies for investigating the economic use of ancient ecotone regions between forest and "prairie" areas. However, as Edwards (1982) cautions, using anthropogenic interpretations of vegetational change during the Early Postglacial times often cannot be documented with any degree of certainty. In fact, during these periods, a natural rather than a cultural explanation is more likely to be correct.

The fossil pollen analysis of ancient floor surfaces in architectural dwellings is yet another way that palynologists can help the archaeologist to understand past environmental and cultural phenomena. One of the first applications of this technique was demonstrated by Schoenwetter (1962) when he used soil scraping from peublo dwelling in eastern Arizona to date the periods of site occupation and infer past climatic conditions. Hevly (1964) attempted a similar study of ancient floor surfaces in abandoned pueblo dwellings located in the Colorado Plateau area of the American Southwest. Hill and Hevly (1968) later attempted to determine room function through the application of fossil pollen analysis in their investigation of the sediments recovered from Broken K Pueblo, Arizona. Unfortunately, these earlier studies did not employ multiple pinch sampling as later suggested by Cully (1979). In addition to occupational floor levels found in structures, agricultural fields are also being examined. Dimbleby (1985) discussed the fossil pollen evidence of Goodburn, at Winterton, Hamberside, England, in which he used palynology to identify possible associated agricultural fields. Wiseman (1983) likewise has recovered fossil pollen data from possible agricultural fields in Lowland Central America.

Under ideal circumstances pollen analyses from archaeological sites can also be used to determine a wide range of other cultural phenomena including: prehistoric diets, possible graveside rituals, subsistence patterns, use of native or cultivated plants, use of certain types of artifacts (*e.g.* grinding stones, pottery, and baskets), probable use of areas within architectural structures, intersite and intrasite dating, and preexcavation recognition of potentially important archaeological sites.

Pollen analyses of archaeological sites are now serving as a useful technique for determining the probable function of certain types of baskets, ceramic vessels, bedrock mortars, and milling stones. Experiments have demonstrated (Bohrer 1968) that pollen can be inadvetently included during the gathering and later storage of certain types of foods such as *Zea* (maize), *Typha* (cattail), *Cleome* (beeweed), *Chenopodium* (goosefoot), and *Amaranthus* (amaranth). Pollen from these plants often adheres to the inside surfaces of baskets and ceramic vessels. Later, when recovered by the archaeologist, these same vessels can be analyzed for their fossil pollen contents and the resulting data can be used to determine probable functional use. Others have noted that the pollen contents on the surfaces of grinding stones often are reflective of the plants utilized (Hevly 1964; Bryant and Morris n.d.).

Under certain circumstances, bedrock mortars can also be examined for their fossil pollen contents. Careful removal of dirt and the subsequent analysis of the fossil pollen trapped at the bottom of these mortars sometimes indicates which plant foods were ground in these mortars. On the other hand, an unfavorable aspect of this type of analysis is that bedrock mortars also may contain modern pollen that was deposited after the mortars were actually in use. Therefore, it is rare that a palynologist is able to determine with certainty the precise food plants which may have been originally processed in bedrock mortars.

Basketry and other woven artifacts sometimes provides important clues to the types of plant material which they carried. Often the dirt trapped between the weaves contains fossil pollen grains that became embedded while the basket was still in use. Like the pollen recovered from the inside surfaces of ceramic vessels, these fossil pollen grains can be used to determine which plant materials were collected and/or stored in basket

containers. Many ceramic vessels made during aboriginal times were used to store food, as cooking pots, and as food containers for meals. If the foods in the vessels contained pollen, then often some of that pollen became trapped along the inside surfaces of these vessels during their use and can be utilized as clues to prehistoric uses of the vessels. Unfortunately, the archaeologist is not always able to test this sampling technique at archaeological sites since many ceramic vessels are recovered only as broken sherds. Hevly (1970), for example, recovered fossil pollen from a mid-Pueblo III age, sealed, ceramic storage jar excavated from a site in northern Arizona. His analysis revealed very high percentages of fossil pine pollen and fungal spores, which he believed represented a post-depositional phenomenon rather than the jar's original juse. In a related study by Bryant and Morris (n.d.) the inside bottom portions of 42 complete ceramic vessels recovered from rooms and burials at the Antelope House Pueblo were scraped carefully to recover fossil pollen that was deposited when the vessels were in use. The fossil pollen spectrum recovered from the bottommost vessel scrapings were statistically compared with the fossil pollen spectrum in the dirt matrix of each vessel. In cases where chi-square tests revealed that the two pollen spectra from the same vessel were of different origin, Bryant and Morris (n.d.) were able to assign probable functional use categories to over 30 corrugated, wide-mouthed vessels dating from the Late Pueblo III period as well as one Late Pueblo III plainware open bowl.

Pollen analysis of soils recovered directly underneath a Neanderthal burial (Leroi-Gourhan 1975) at Shanidar Cave, Iraq, revealed unusually high percentages of pollen from tiny, insect-pollinated alpine flowers. Because of the low pollen productivity of zoophilous plants, it was assumed that the high percentages of those pollen types in the burial soils must have resulted from cultural, not natural introduction. Therefore, Leroi-Gorhan (1975) concluded that a large number of small, alpine flowers must have been collected from the nearby hillsides and then carefully placed in the Neanderthal grave at the time of internment. The significance of this research has had a profound effect upon our understanding of Neanderthal's cultural activities and the possibility that they were the first group to adopt a basic philosophy about religion and the afterlife.

At Broken K Pueblo in Arizona, Hill and Hevly (1968) noted the probable ceremonial use of *Sphaeralcea* (mallow) and pine pollen in an infant's burial. In a nearby region of Arizona, Bryant and Morris (n.d.) noted what they suspected was a similar use of maize pollen as part of a graveside ritual at Antelope House Pueblo during the internment of a young child. These two studies, each from different pueblo sites, suggest that in some Southwestern groups flowers and pollen may have been thrown into the graves of the dead as some type of mortuary ritual accompanying internment. More importantly, at each of these burial sites, it was the fossil pollen data that helped establish new cultural insights into our understanding of prehistoric rites.

The pollen analysis of preserved human coprolites is one of the most useful methods for determining prehistoric diet and certain types of cultural patterns. Pollen data recovered from human coprolites can, under ideal circumstances yield information as to: (1) the use and source of economic and background pollen types; (2) the seasonality of site usage; (3) diet; and (4) certain aspects concerning paleoenvironmental conditions (Bryant 1974). However, like other forms of pollen data, the best and most reliable records come from studies at a given site in which the data base consists of many coprolites all dating to the same time period, rather than from only a few or even a single specimen representing one stratum or one time period. Single coprolite specimens, like a single lithic artifact, may or may not be an accurate reflection of the entire time period represented by the stratum.

Pollen studies of coprolites have shown that not all of the pollen found in coprolites reflect aspects of human diet. Within each human coprolite there are two distinct groups of pollen: economic pollen and background pollen. Economic pollen, as a specific category,

is recognized by palynologists to include those pollen types that were probably ingested directly as part of the diet. Most of these economic pollen types are easily recognized because they come from plant species having pollen that is rarely, if ever, found as part of the normal atmospheric pollen rain. Economic pollen grains from zoophilous plant such as *Cleome*, *Yucca* (yucca), *Opuntia* (cactus), *Agave* (agave), *Dasylirion* (sotol), *Cucurbita* (squash), and *Prosopis* (mesquite) are transported from flower to flower by insects and thus rarely become airborne in the atmosphere as do the wind pollinated background grains of *Ambrosia* (ragweed), *Pinus* (pine), or *Quercus* (oak). Therefore, when significant quantities of zoophilous economic pollen types are found in human coprolite specimens, they are generally interpreted to reflect the direct consumption of the plant, flowers, or honey made from those plants rather than representing the accidental ingestion of these pollen grains from atmospheric sources. Some wind pollinated types of pollen such as *Chenopodium*, *Zea mays*, *Iva* (marsh elder), *Ephedra* (mormon tea), *Typha* and *Helianthus* (sunflower) may or may not be included as economic types depending upon the circumstances under which they are found. Since these types come from edible plants whose pollen often adheres to the collected seeds of these plants or to other plant parts which are known ethnographically to have been utilized, the presence of these pollen types can often be associated with economic use rather than representing deposition from strictly natural pollen rain sources.

The preserved pollen in human coprolites sometimes may offer clues as to the probable seasonality of site occupation and insights concerning the paleoenvironment at the time when the coprolite was produced. When human coprolites contain high percentages of economic pollen from one or more plants that generally bloom during the spring and/or summer, then that information can be used to offer tentative evidence for seasonal usage of the site. Ethnobotanical reports of many subsistence-level protohistoric cultures in North America have noted that in most instances, flowers were eaten fresh and that honey was rarely, if ever, found or eaten (Barrows 1900; Castettler and Bell 1941; Curtin 1949; Newberry 1887; Palmer 1887) thereby confirming that flower pollen can generally be attributed to seasonality when recovered in coprolites. In a similar way, the presence of coprolitic pollen from plants which no longer inhabit the region may suggest that environmental changes have occurred which caused the extinction or out migration of certain plant taxa.

From this somewhat cursory overview, it is evident that palynological investigations of anthropogenic issues require asking specific types of questions dealing with both cultural and botanical science and the understanding of both anthropological and botanical research methodologies (King 1985). Furthermore, we believe that one of the major stumbling blocks that discourages the incorporation of palynological data into archaeological research has been the use of techniques which are generally employed in stratigraphic palynology to answer non-stratigraphic types of culturally important questions. Many times, this type of palynological approach is just not applicable to solving anthropogenic issues. In attempting to formulate testable hypotheses concerning anthropological questions, palynologists must be prepared to discard precepts which are almost inviolate in biostratigraphy. For example, in some cases valuable information can be obtained solely by the use of presence/absence criteria in archaeological palynology. In such instances, these arguments may provide a better basis for data explanation and synthesis within the framework of archaeological concepts.

DATA ANLYSIS AND STATISTICS

Data analysis and interpretation are two aspects that have become rapidly more sophisticated during the past two decades. More than 60 years ago, von Post (Davis and Faegri 1967) based his initial pollen analysis on calculated, relative pollen frequencies.

Although in many cases this type of statistic is still useful, it lacks the needed precision for most of today's studies.

Throughout the first half of the twentieth century, relative pollen frequencies were used exclusviely by palynologists both in the reconstruction of paleoenvironmental conditions and in the interpretation of palynological data from archaeological sites. After the introduction of radiocarbon dating and the availability of tightly controlled temporal chronologies, Benninghoff (1962), and later Davis (1963, 1966, 1969), Jorgensen (1967), and Matthews (1969) helped to develop new methods for the calculation of fossil pollen ratios through the use of pollen concentration and pollen influx values. Although pollen influx data provide needed additional information used in paleoenvironmental reconstructions, the lack of precise stratigraphic and temporal control within an archaeological site often precludes the use of pollen influx values in these sediments. Therefore, in order to provide more meaningful results from the analysis of pollen data recovered from archaeological sites, palynologists have begun to use pollen concentration values which can be expressed in terms of the number of fossil grains per unit weight or volume, and then those ratios can be compared between levels of the same site or between similar cultural strata from different sites. This type of comparison has proven useful for demonstrating smilarities and/or differences in the fossil pollen record. Although concentration values can be computed using either sample weight or volume, we have found that the volume method is more reliable for comparing both intrasite and intersite pollen variability (Bryant and Holloway 1983).

While multivariate statistical analysis of palynological data from archaeological sites can provide a wealth of information, the major utility of statistical analyses has been realized within the framework of biostratigraphic and paleoecological problems. Initially, much of the work was, and still is, concerned with statistically analyzing modern pollen assemblages and using these data to infer paleoecological changes. Initially, multivariate statistics were utilized quite effectively to quantify the relationship between surface pollen and vegetation (Webb and Bryson 1972; O'Sullivan and Riley 1974; Birks *et al.* 1975; Webb and McAndrews 1976; Bernabo and Webb 1977; among others). Much of this research was aimed at obtaining modern analogues to fossil pollen assemblages. However, as many of these authors noted, some assemblages, especially those from late-glacial/Holocene times, lacked modern analogues and thus were outside the purvue of these techniques. Recently, Liu and Lam (1985) using discriminat analysis were able to statistically quantify these anomalous assemblages.

In an attempt to reconcile surface pollen spectra with the extant vegetation, Davis and Goodlett (1960), earlier had devised the concept of R-values. Although a valid attempt at solving these problems, the model was never universally accepted primarily because, as noted by Livingstone (1968, 1969), of the model's failure to accurately reflect over-representation of arboreal pollen types. Recently, Parsons and Prentice (1981:127) demonstrated the utility of Davis and Goodlett's (1960) model "subject to certain caveats." Parsons and Prentice (1981) developed three mathematical models to account for the observed discrepancies. These mathematical models involved the use of principal components analysis and regression equations which provide a good basis for inferring vegetational compositon from surface pollen data.

Regression analysis has likewise been utilized recently (Webb *et al.* 1981; Heide and Bradshaw 1982) to estimate directly the plant abundance of vegetational stands based on their surface pollen rain. Delcourt and Delcourt (1984) investigated the distribution of 24 major tree taxa and produced paired maps showing the percentage of growing stock volume (isophyte maps) and arboreal pollen percentages (isopool maps). These data were produced using a geometric-mean linear regression and have proved valuable in the quantitative reconstruction of vegetational change in eastern North America.

These attempts to correlate vegetational composition with surface pollen rain have been supported largely by the impetus of research projects such as CLIMAP (1976, 1981) which have utilized paleoenvironmental data to reconstruct ancient landscapes. Almost from the beginning of these studies, palynologists recognized the importance of ultimately relating the pollen/plant relationships to the interpretation of climatic data (Webb and Bryson 1972; Webb and Clark 1977; Kay 1979; Andrews *et al.* 1980; Webb 1980; Heusser and Streeter 1980). More recently, Howe and Webb (1983) have effectively discussed the methodologies employed in calibrating palynological data in terms of quantitatively derived climate estimates. These procedures can be combined with certain mapping techniques (Webb 1983) to provide measurements of changes in paleovegetational communities.

These types of paleoenvironmental data, although derived from differing sources, have been utilized by Delcourt and Delcourt for the production of statistical models reflecting biotic responses to climatic patterns occurring throughout the Holocene. Drawing upon the established data base, Delcourt and Delcourt (1983) have additionally provided a model designed to test vegetational responses to climate changes occurring at various periods within the Holocene. The model is exactly what is needed as it provides mechanisms to predict future modifications of the biota in response to those climate changes induced by man (*i.e.*, the estimated global warming trend caused by increased CO_2 concentrations in the atmosphere). These types of quantitative models serve as generalized hypotheses for empirical testing.

These techniques and approaches cannot, at this stage, be applied universally. The quantitative measurements have been derived primarily from sites located within the Upper Midwest, or southeastern U.S. The work in these geographical areas has been conducted for several decades. Not only are a large number of palynological sites available for study (Holloway and Bryant 1985; Delcourt and Delcourt 1985) from these areas, but extremely large numbers of surface samples and the corresponding vegetational data have been collected. It is only in those regions where a large accumulated data base is available that multivariate statistical analyses of data can provide both the models and the empirical tests for vegetational responses to climate.

The ultimate goal of paleoenvironmental reconstruction is, of course, to draw on as much data as possible and then use those data for determining comparisons between adjacent geographical areas. Gordon and Birks (1974) attempted to statistically delimit pollen zones (*i.e.* a series of adjacent levels with similar pollen assemblages) and then use those zones for chronological purposes within a given geographical region. The advantage of using such pollen zones is that they permit comparisons of two or more pollen records from different locations within a given geographical locale. The main disadvantage of using pollen zones is that their selection must be unbiased which argues for a numerical approach rather than an intuitive approach to the problem.

Numerous statistical techniques are presently available for numerical analysis and zonation of pollen data. Birks and Gordon (1985) recently have summarized many of these techniques and have provided computer programs for general dissemination. While these analytical techniques are readily available, few palynologists routinely use them. What is important to remember is that these numerical techniques were not designed to replace the interpretation of the palynologist, but rather to insure that the observed changes are real (not biased), and that a significant amount of variation is present before interpreting drastic changes.

Although as a general rule paleoenvironmental reconstructions from archaeological sites pose severe conceptual restrictions due to the biased nature of the data base, a numerical approach has been demonstrated which may alleviate this problem. Fall *et al.* (1981) have utilized principal components analysis to extract paleoenvironmental data from Antelope House in Arizona. Based on over 100 pollen samples, the numerical method

reflected changes in vegetation patterns through time which were correlated with environmental indices reflecting temperature and moisture. These were likewise interpreted in light of known cultural activity patterns and population movements (Fall *et al.* 1981).

As Fall *et al.* (1981) correctly observed, in many archaeological sites, especially in the American Southwest, most palynologists have difficulty in distinguishing which pollen represents the naturally occurring regional pollen rain and which types reflect the cultural activities of man. In their study, the authors, using multi-variate statistical techniques, were able to successfully separate these two components. Once assured of the composition of the regional pollen rain, they demonstrated the effectiveness of these numerical techniques for the interpretation of paleoenvironmental conditions and in some cases, the human response to these conditions.

SUMMARY

During the past fifty years the fields of archaeology and palynology have begun to establish a working relationship in which the scientists of each discipline have tried to help the other understand what is needed in archaeological palynology. In some cases this has succeeded quite well, but in others it remains a problem. Over the years the primary problem between workers in these two disciplines has been effective communication. Archaeologists have moved into the area of hypothesis testing at many of their sites while most of the palynologists working with archaeologists are still in the data collection stage. Other problems have developed when archaeologists have unrealistic expectations from the palynological data and have become disappointed when fossil pollen information was unable to solve some major hypothesis or problem confronting the archaeologist. Additionally, there has been a great deal of misunderstanding on the part of both archaeologists and palynologists as to exactly what information is needed from the fossil pollen data at a site. One example of this should serve as an illustration of what we mean.

During the recent excavation and fossil pollen study of an archaeological site, a palynologist presented a wide array of interpretations as to what the prehistoric inhabitants may have been using as food, the types of plant materials they carried into the site and later used in the making of matting, clothing, and other items of their material culture. It was a carefully researched study and presented the types of information that most archaeologists would be pleased to obtain from fossil pollen studies of their site sediments. However, at this particular site, the sediment samples which had been examined and interpreted by the palynologist all came from culturally sterile strata which contained no artifacts or any other evidence suggesting that the site was even occupied by prehistoric groups during the deposition of those strata.

Another area in which communications between palynologists and archaeologists have been lacking is in the application and use of statistics. During the past several decades archaeologists have been quick to apply the use of statistical methods to studies of the cultural remains from their sites; however, until very recently most fossil pollen studies of archaeological sediments have not made effective use of new methodology in statistics. Hopefully, in the decades to come we will see an increased emphasis placed upon the use of statistical methods such as multivariate statistical-, principal component-, and regression-analysis when working with the pollen data from archaeological sites. In the past these statistical methods have been used in the analysis of pollen data from a few archaeological sites and have proved to be an effective method of interpreting information. These methods of data analysis and statistical approaches represent some of the currently utilized techniques in studies of archaeological palynology. However, there are many other alternative approaches from the wider field of palynology which cur-

rently have limited application. Perhaps, as archaeological palynologists become more comfortable with the use of statistics as they apply directly to problems in archaeological palynology, then new avenues of data integration will develop which will provide a wide range of new information on which to base interpretations.

Also, further studies are needed in the area of pollen degradation and in the better understanding of pollen sources which become trapped in archaeological sites. Unlike most peat bogs, lakes, and other terrestrial deposits where the activities of mankind are not a factor, the pollen in archaeological sites comes from both natural sources and from the activities of cultural groups. As shown earlier, there are many ways in which the activities of cultural groups can alter the natural vegetation through burning, agricultural or pastoral practices, through the selective use of certain firewoods or plants while others are not disturbed, and through the collection and use of certain plants to make their shelters or clothing. Degradation of fossil pollen in archaeological deposits is also an important issue. It is well to remember there are many factors which will lead to the destruction of pollen. In addition, experiments have shown that not all types of pollen grains degrade at the same rate, therefore, selective destruction of fossil pollen types can occur. That, in turn, will create a preserved pollen record that may be quite different from the actual pollen rain that was originally deposited. Knowledge of these differences are critical to the preparation of meaningful pollen analyses pertaining to archaeological sediments.

Finally, one aspect which we hope will be corrected in the years to come is the wide variety and range of archaeological fossil pollen records which currently are being buried in various "contract-type" reports. What makes this problem especially troublesome is that all too often they are printed in very limited numbers and are not widely distributed. If this procedure continues during the next decades, it will contribute to the unintentional duplication of fossil pollen research and will hinder researchers in a given area from being able to utilize the full range of fossil pollen data which may be available only in numerous contract-type reports. What is desperately needed in the field of archaeological palynology is a national or regional clearing house for these types of reports. In this way, the results of completed research will be available to the entire field.

The union of archaeology and palynology has been accomplished and now most archaeologists consider fossil pollen data from their sites as being an important aspect for them to consider. Furthermore, during the past several decades more and more palynologists with strong backgrounds in both botany and archaeology are entering the profession and are now in a position to communicate effectively with archaeologists. In addition, more and more pollen analyses are now becoming available from archaeological deposits in a wide variety of geographical areas. In other words, the data base is expanding rapidly. Soon, it is hoped that the archaeological palynologist will be able to turn his or her attention more toward research involving the testing of specific hypotheses, and away from the simple data collection.

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ON THE ANALYSIS AND INTERPRETATION OF SPECIES LIST DATA IN ZOOARCHAEOLOGY

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ABSTRACT.—Zooarchaeologists regularly discuss methods of quantifying faunal remains, but seldom explore the information potential found in species lists. Late Pleistocene and early Holocene mammalian species lists derived from sites in eastern Washington indicate that species list data can provide insights to details of historic zoogeography and paleoenvironmental conditions. The presence of particular taxa and the climatic regime in a region are both factors to which prehistoric people adapted, and thus play critical roles in building models of human settlement, subsistence, and land use systems.

INTRODUCTION

Discussions of how to quantify vertebrate faunal remains recovered from archaeological sites have reached a point where the specialist and novice zooarchaeologist alike must read and digest a tremendous volume of literature (see Allen and Guy 1984; Binford 1984; Grayson 1984; Horton 1984; Klein and Cruz-Uribe 1984; Lyman 1984b; Nichol and Wild 1984 for a single year of publications). Upon cursory study of this literature it might appear that in order to do zooarchaeological research, the analyst must not only decide how to count the faunal remains, but must also have a large bone sample. Statements that species lists are of little analytic and interpretive value (e.g., Smith 1976) only serve to strengthen this notion. In this paper I illustrate that such impressions are far from true by analyzing and discussing the implications of late Pleistocene/early Holocene mammalian species lists. While the intent of my discussion is to show that large samples and taxonomic abundance data are not always necessary in zooarchaeology, it is not my intent to show the converse—that large samples and abundance data are never necessary. It should be clear at the outset that the amount as well as kind of data required to answer a particular research question depend, of course, on the question asked.

DISCUSSION

The potential for deriving significant information from small faunas, rare taxa, and species lists has been recognized before. In one of the more important discussions of that potential, Grayson (1981a) argued that because of the taphonomical difficulties inherent in ascertaining the paleoenvironmental meaning of the relative abundance of taxa, the analyst may choose to simply use the ecological attributes of the taxa represented in a fauna as the basis for interpretations of paleoenvironments. The power of such interpretations lies in their parsimony; there are fewer assumptions about the taphonomic history of the fauna when taxa are treated as attributes of a fauna that are either present or absent in contrast to treating those taxa as variables whose abundances are the basis of interpretations (see also Grayson 1984).

Grayson (1981b) illustrates the elegance of the "taxa as attributes" approach by using two archaeological specimens (probably from the same individual animal) of the heather vole *(Phenacomys* cf. *intermedius)* from a single stratum sample of 406 specimens to corroborate an historic biogeographical model proposed for Great Basin mammals (Brown 1971, 1978). Those same two heather vole specimens, along with other faunal and floral taxa treated as attributes are used by Thompson and Mead (1982) as bases for their inferences regarding late Pleistocene climatic conditions in the Great Basin.

Clarifying the zoogeographic history of particular taxa (e.g., Lyman 1983) can have important zoological implications. It is seldom explicit, however, in studies of prehistoric human subsistence systems, that taxa *must be present* in an area to be exploited by humans (see Lyman 1984a for an example where this point is explicitly made). Once this fact is recognized, the utility of understanding the zoogeographic histories of potentially exploitable prey animals to studies of human subsistence becomes obvious. As well, when those zoogeographic histories have environmental and climatic implications (e.g., Lyman and Livingston 1983), their significance for gaining a fuller understanding of human settlement systems and land use practices increases. Studies by Binford (1980, 1982) and Kelly (1983), for example, indicate that the degree of mobility in a human land use system may be closely tied to resource availability and climatic factors. Simply put, then, faunal data of even statistically low-resolution (nominal) scale can be important to archaeological research.

EXAMPLES

In order to elaborate on and illustrate the above points, I have chosen late Pleistocene and early Holocene faunas recovered from sites in eastern Washington (Fig. 1). The obvious reason for this choice is that this is the area with which I am most familiar (Lyman and Livingston 1983). The second and more important reason to use examples from this area and time period is based on issues of sample size and what we know of the faunal history of the area. Quite literally, we know very little of the latter, so this paper becomes a substantive contribution. Concerning the sample size issue, only five mammalian faunal samples that are early Holocene in age are available in this area (Table 1). As well, only three late Pleistocene mammalian faunas have been described for eastern Washington. In contrast, over a dozen middle Holocene and more than fifty late Holocene mammalian archaeofaunas have been reported. Further, less than 2000 identified specimens make up six of the late Pleistocene/early Holocene samples, while several late Holocene faunas have thousands of specimens, and several middle Holocene faunas are larger than most early Holocene faunas (Table 1). Clearly, the rule of thumb that the closer to the present we are in time the more we know holds here.

Can we learn anything about late Pleistocene/early Holocene faunas and the early ecology of eastern Washington from the available data? I think we can, given the taxa represented in each of the faunas I have chosen to examine (Table 2). For clarity, I have divided the following into two sections. First, I explore the implications of the historical zoogeography of several taxa for human subsistence practices. Then, I discuss the climatic implications of two taxa, and how the inferred environment might have affected human land use practices.

Historical zoogeography.—The species lists in Table 2 illuminate certain aspects of the late Quaternary historical zoogeography of eastern Washington. For example, the Jeppson Locality fauna, the Kennewick Roadcut fauna, and the Umatilla Mammoth Site fauna provide an unprecedented view of the Wisconsinan-age mammalian biota of the area (cf. Kurten and Anderson 1980:42-43). With these faunas we find that the mammoth

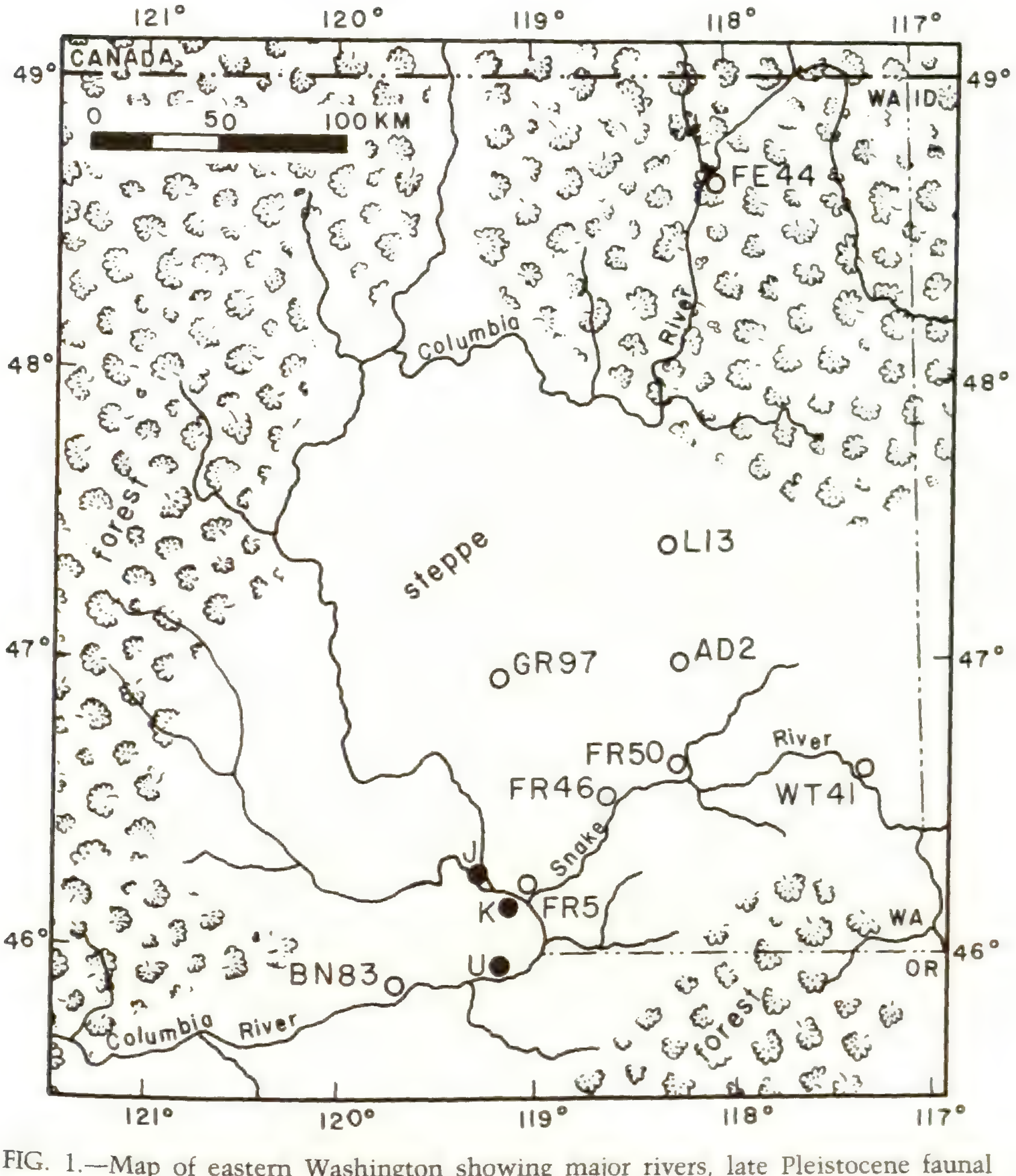

FIG. 1.—Map of eastern Washington showing major rivers, late Pleistocene faunal localities (dots: J — Jeppson Locality, K — Kennewick roadcut, U — Umatilla Mammoth Site), Holocene archaeological sites (circles), and modern distribution of forested and steppe habitats.

(*Mammuthus* sp.) was present in eastern Washington near the end of the Pleistocene, or circa 13,000 B.P. This is a detail that has until now only been suspected (Agenborad 1984; Fry 1969; Newcomb and Repenning 1970). The significance of this detail for the earliest human occupants of eastern Washington is as yet unclear because no evidence of humans associated with mammoths has been found in the area.

Other noteworth details about the late Pleistocene species lists are the presence of musk ox (*Bootherium* sp.), pronghorn antelope (*Antilocapra americana*), northern grasshopper mouse (*Onychomys leucogaster*) Nuttall's cottontail (*Sylvilagus nuttallii*),

and Townsend's ground squirrel *(Spermophilus townsendii)*. Dalquest (1948) postulated that the latter four taxa entered eastern Washington from an eastern Oregon refugium during post-Wisconsinan times (see also Lyman and Livingston 1983). The available data indicate that these four taxa had at least a limited distribution in the area during the latest Wisconsinan, and thus would have been available for exploitation by early human hunters. Bison *(Bison* sp.) were also available throughout the Holocene (Lyman and Livingston 1983), becoming locally extinct only during the late prehistoric/early historic period.

TABLE 1.—*Descriptive data for selected late Pleistocene and Holocene mammalian faunas from eastern Washington.*

Site	No. of Taxa	No. of Identified Specimens	Age in Years B.P.	Reference
Late Pleistocene:				
Jeppson Locality	9	318 +	13,000	Martin et al. 1982
Umatilla Mammoth Site	4	unknown	13,000	Gilbow 1981
Kennewick Roadcut	16	about 1000	ca. 30,000 to 10,000*	Renseberger et al. 1984
Early Holocene:				
Kennewick Roadcut	3	about 30	ca. 10,000 to 7000*	Renseberger et al. 1984
45FE44	9	42	9000-5000	Chance and Chance 1982
45GR97	17	319	8700	Irwin and Moody 1978
45FR50	13	59	10,000-7500	Gustafson 1972
45FR46	6	491	8500-8000	Mead et al. 1984
Middle Holocene:				
45L13	17	432	mid-Holocene	Bryan 1955
45FR50	11	427	7500-4000	Gustafson 1972
45WT41	17	365	8000-5000	Gustafson 1972
45FR46	13	359	8000-4000	Mead et al. 1984
Late Holocene:				
45BN83	9	2119	3100-3000	Cole 1968
45AD2	28	3128	3500-300	Deaver and Greene 1978
45FR5	16	4748	500	Olson 1983

*estimate

TABLE 2.—*Species lists for three late Pleistocene and five early Holocene mammalian faunas in eastern Washington.*

TAXON	Late Pleistocene			Early Holocene				
	Jeppson	Umatilla	Kennewick RC	Kennewick RC	45FE44	45GR97	45FR50	45FR46
Sorex palustris			+					
Castor canadensis					+	+		
Lepus sp.	+		+			+	+	
Marmota flaviventris						+	+	
Cricetidae								+
Microtinae	+				+	+		
Microtus sp.			+					+
Lagurus curtatus			+	+				
Neotoma sp.								+
Neotoma cinerea					+	+	+	
Ondatra zibethicus					+	+	+	
Onychomys leucogaster	+							
Perognathus parvus	+		+			+		+
Peromyscus maniculatus	+		+			+	+	
Eutamias sp.			+					
Spermophilus sp.								+
Spermophilus columbianus						+		
Spermophilus townsendii	+		+	+				
Sylvilagus nuttallii			+			+	+	
Sylvilagus idahoensis			+					
Thomomys talpoides	+		+	+	+	+	+	+
Alopex lagopus							+	
Canis sp.					+		+	
Martes americana							+	
Mephitis mephitis						+		
Taxidea taxus						+		
Ursus sp.					+			
Ursus arctos					+			
Vulpes vulpes					+	+		
Antilocapra americana		+					+	
Bison sp.						+		
Cervus elaphus						+	+	
Odocoileus sp.		+				+	+	
Mammuthus sp.	+	+	+					
Bootherium sp.		+						

The presence of 9000 year old grizzly bear *(Ursus arctos)* remains in northeastern Washington conforms to Dalquest's (1948) conjecture that this taxon entered the area in post-Wisconsinan times. There is limited evidence that grizzlies occupied a larger range in eastern Washington during the Holocene than this taxon did in the historic period (Lyman 1986), but additional details on the zoogeographic history of this taxon are unknown.

The above examples indicate that any model of human adaptations during the latest Pleistocene and earliest Holocene cannot simply be based on modern faunal data. Mammoths, musk ox, bison, antelope, and grizzly bears are all locally extinct at present, and may never have been very abundant in eastern Washington. And yet, because these taxa constitute some of the largest mammals exploited by prehistoric North American peoples, their presence alone warrants careful modeling of early Holocene human adaptive strategies.

Hayden (1981) has argued that human subsistence can be precarious in areas where the richness (number) of taxa present is low because overexploitation of one taxon may stress the entire ecosystem, including the human element, and natural fluctuations in the abundance of one taxon may also cause stress because of the limited number of alternative prey species. Are the known faunas dating to the late Pleistocene and early Holocene more or less taxonomically rich than the middle or late Holocene faunas of eastern Washington? While there is no statistically significant correlation of sample size (NISP) and taxonomic richness for the faunas listed in Table 1 (Kendall's tau = 0.119; $p > 0.2$; two-tailed test), given the available data it is difficult to answer this question. It appears that Gustafson's (1972) suggestion of over a decade ago that the mammalian taxonomic composition of eastern Washington during the earliest Holocene was not appreciably different from the late Holocene fauna of that area cannot be seriously challenged (see also Lyman and Livington 1983). The taxonomic composition of eastern Washington's mammalian fauna apparently changed during the late Pleistocene to early Holocene transition (e.g., loss of *Mammuthus* sp., *Bootherium* sp., and *Alopex lagopus*, with coincident addition of *Antilocapra americana*, *Onychomys leucogaster*, and *Spermophilus townsendii)*, but species richness may not have changed significantly. The focus of research on human subsistence should then change from monitoring simple species richness to taxonomic composition; that is, determining which taxa were present, and ascertaining how those taxa might have most efficiently been exploited. For example, successful hunting techniques for taking mammoths may not be appropriate for efficient exploitation of pronghorn antelope (see for instance the discussion in Frison 1978).

Ecological zoogeography.—Late Quaternary environmental conditions in eastern Washington have been postulated on the basis of palynological data (Mehringer 1985 and references therein). Essentially, the late Pleistocene to early Holocene transition is reflected by climatic warming in conjunction with a gradual decrease in effective precipitation. Nonetheless, the environment about 10,000 years ago seems to have been generally cooler and moister than at present.

Interpreting the mammalian zoogeographic data for eastern Washington in climatic terms, Lyman and Livingston (1983) simply tracked the distributions of selected taxa across environmental and faunal zones defined by Dalquest (1948). They suggested the zoogeographic data broadly reflected the environmental history suggested by the palynological data for this area (see also Lyman 1980, 1984a, 1986). There is what might be thought of as a more direct technique for measuring the environmental significance of zoogeographic data, and it is that technique I will now illustrate.

A climatograph (Graham 1984) can be constructed for any taxon with a limited distribution by recording the temperature and precipitation regimes at a series of points plotted at the edge of the taxon's modern range. While it is clear that many variables control a taxon's distribution, such as interspecific competition and historical events (see reviews in Brown and Gibson 1983 and Pielou 1979), "many of these variables are dependent on or interface with climatic parameters" (Graham 1984:111). As well, regardless of how we measure it, the present distribution and resultant *apparent* ecology and environmental tolerances of taxa constitute the basis of all interpretations of past environments derived from prehistoric faunal remains.

Two taxa in the early Holocene faunas of eastern Washington have modern distributions that are particularly conducive to constructing climatographs. These are the 10,000 year old arctic fox *(Alopex lagopus)* from 45FR50, and the 8700 year old Columbian ground squirrel *(Spermophilus columbianus)* from 45GR97. To construct climatographs for these taxa, I plotted ten points around the modern range of each taxon (Figs. 2 and 3). Climatic data were derived for each plotted point on each taxon's range and both archaeological sites by consulting Hare and Thomas (1979) and Visher (1954) (Tables 3 and 4). The climatic data were then plotted on graphs with temperature on the vertical axis and precipitation on the horizontal axis (Fig. 4 and 5).

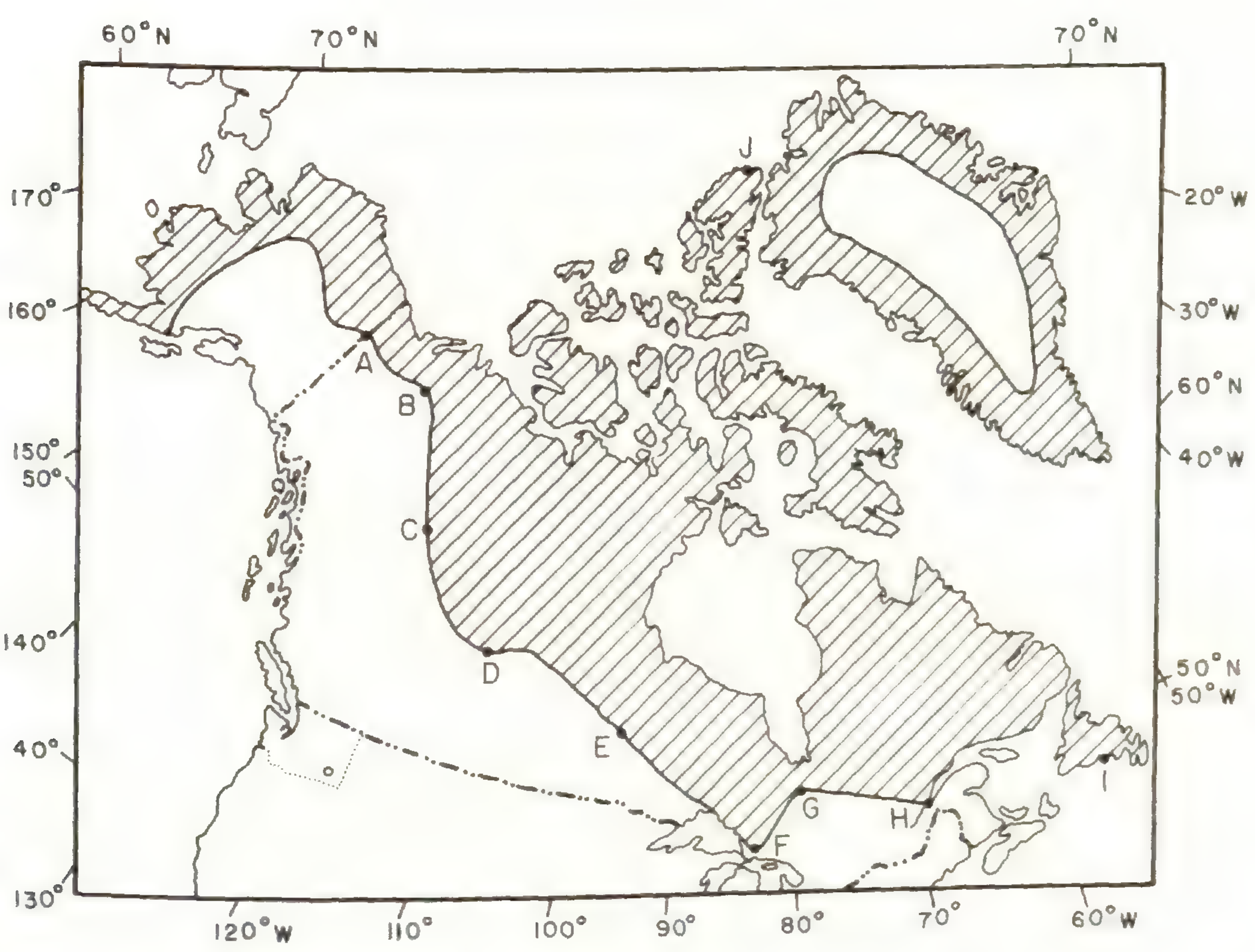

FIG. 2.—Modern distribution of arctic fox *(Alopex lagopus)* in North America (cross-hatched) (after Hall 1981), points where climatic data were recorded (dots and capital letters; see Table 3), and location of 45FR50 (open circle in southeastern Washington) where remains of this taxon have been found in 10,000 year old sediments.

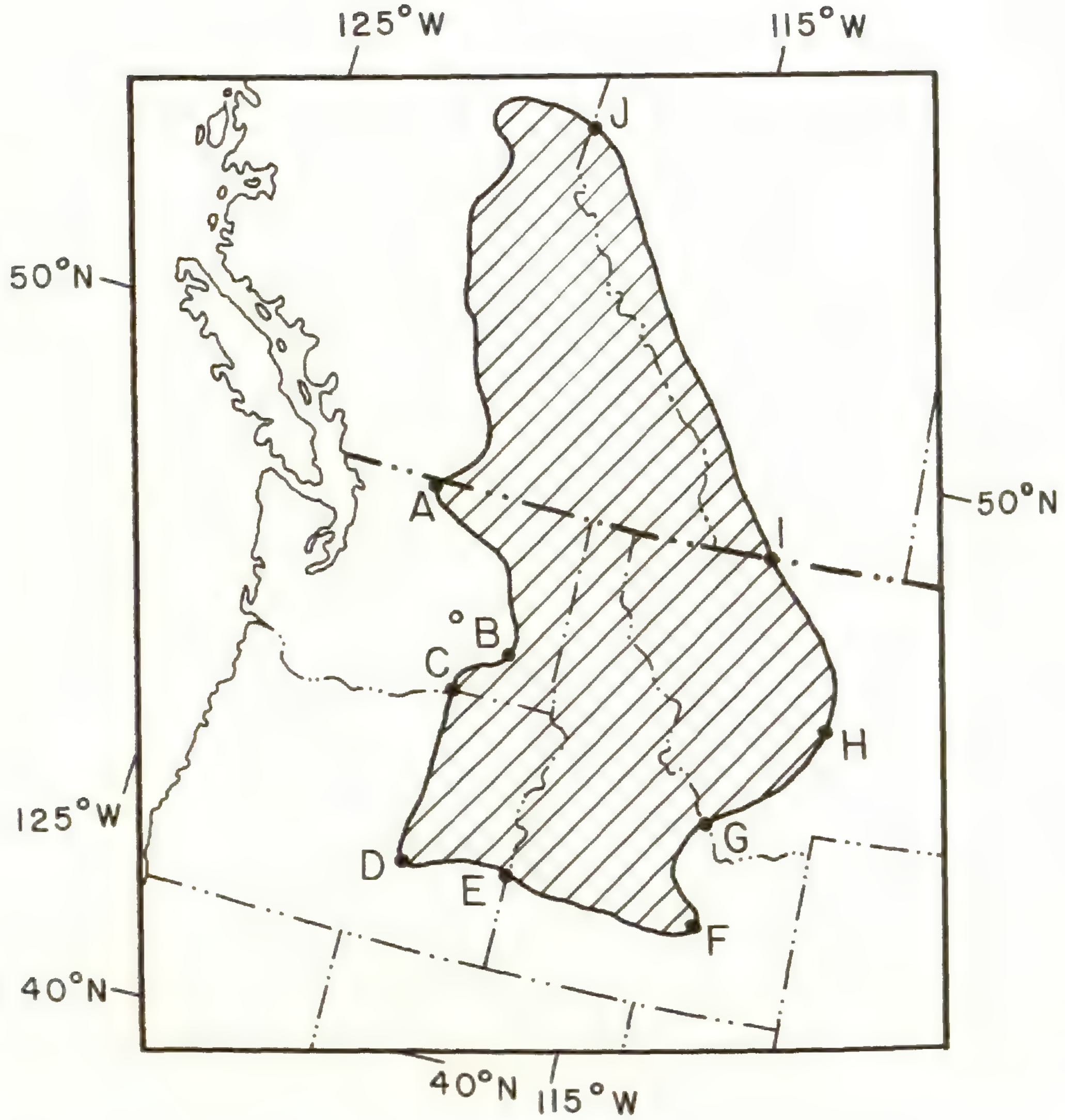

FIG. 3.—Modern distribution of Columbian ground squirrel *(Spermophilus columbianus)* in North America (cross-hatched) (after Hall 1981), points where climatic data were recorded (dots and capital letters; see Table 4), and location of 45GR97 (open circle in central Washington) where remains of this taxon have been found in 8700 year old sediments.

The type of climatic data available for the range of the arctic fox dictated the resulting climatograph; specifically, no seasonal precipitation data were available, and only seasonal temperature data were available (Table 3). The climatograph is nonetheless informative, and suggests summer temperatures would have had to be about 1°C cooler than at present for arctic foxes to have lived in southeastern Washington (Fig. 4). Underwood and Mosher (1982) suggest that the population size of this taxon seems to be dictated by the size of the rodent populations upon which arctic foxes prey, and that arctic foxes are opportunistic feeders and climatic factors may only secondarily control their abundance and distribution as some races can tolerate relatively warm, mild winters. The climatograph and the observations on the modern distribution of arctic foxes tend to

TABLE 3.—*Climatic data for the distributional limits of the arctic fox (Alopex lagopus) and 45FR50. See Figs. 2 and 4.*

Point	January Temperature (°C)	July Temperature (C°)	Annual Precipitation (mm)
A	-30	15	203
B	-30	15	305
C	-25	15	305
D	-20	17	406
E	-20	17	508
F	-12.5	17.5	813
G	-20	17	813
H	-12.5	15	1000
I	-2.5	15	1000
J	-35	5	102
45FR50	1.5	18	381

TABLE 4.—*Climatic data for the distributional limits of the Columbian ground squirrel (Spermophilus columbianus) and 45GR97. See Figs. 3 and 5.*

Point	Temperature (°C)			Precipitation (mm)		
	January	July	Annual	January	July	Annual
A	-1	21	7	152	101	508
B	-1	21	10	101	101	508
C	1.5	21	10	76	101	508
D	-1	18.5	7	76	101	508
E	-4	21	7	101	101	508
F	-6.5	21	4.5	101	101	508
G	-6.5	15.5	1.5	101	101	508
H	-4	18.5	4.5	101	127	508
I	-9.5	15.5	4.5	101	127	508
J	-15	15	ND	100*	100*	508
45GR98	-1.5	18	10	45	0	254

ND; no data available.
*values are estimates.

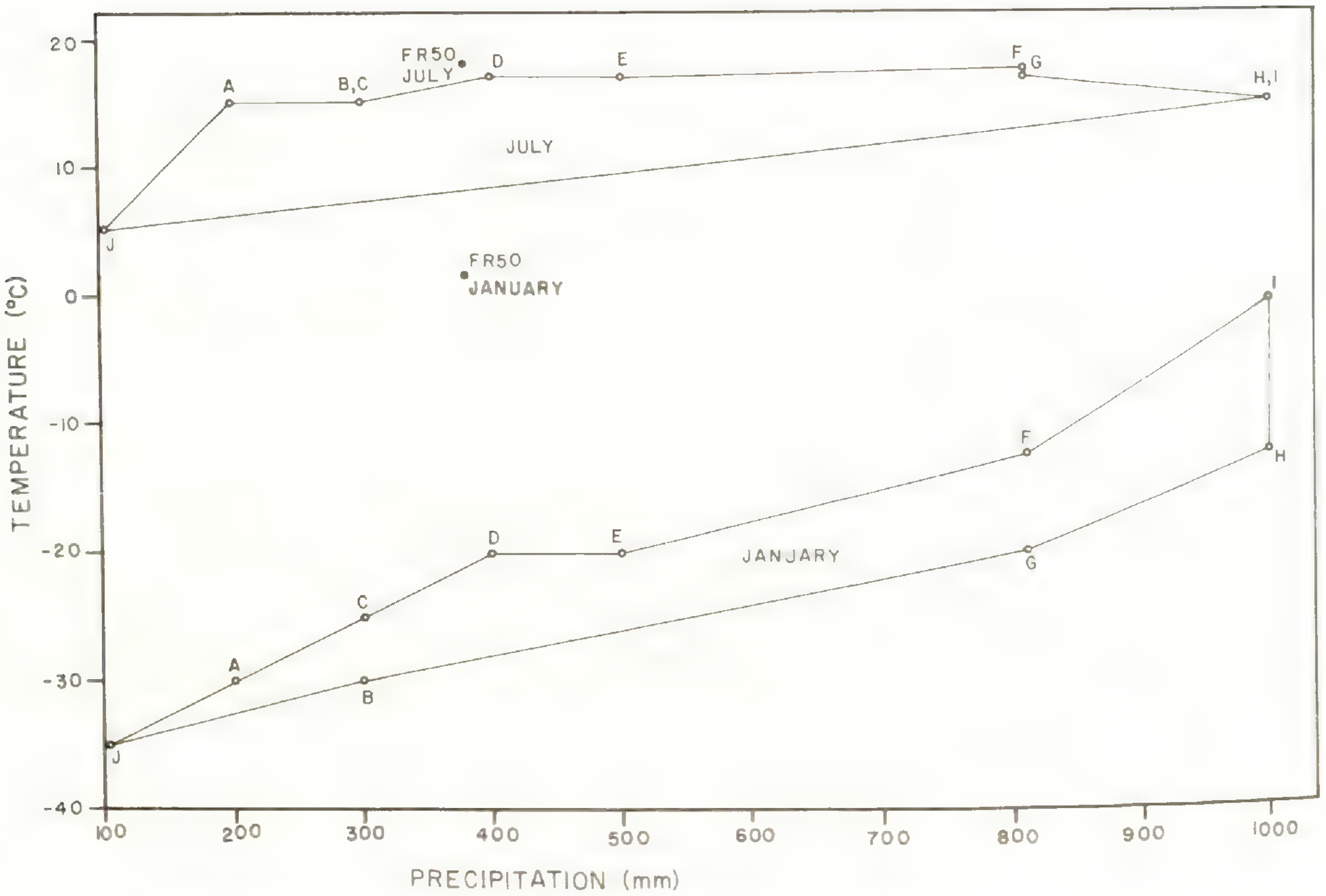

FIG. 4.—Climatograph for arctic fox *(Alopex lagopus)* derived from data in Table 3. The open circles with letter labels are the geographic points (Fig. 2). The upper part of the graph represents July temperatures plotted against annual precipitation; the lower part of the graph represents January temperatures plotted against annual precipitation. The upper dot is the July temperature plotted against annual precipitation for 45FR50; the lower dot is the January temperature plotted against annual precipitation for 45FR50.

corroborate Gustafson's (1972) original interpretations of earliest Holocene environments of eastern Washington as involving milder winters and cooler summers than at present.

The type of climatic data available for the distribution of the Columbian ground squirrel (Table 4) also dictated the nature of the resulting climatograph (Fig. 5) for this taxon. Clearly, it is much too xeric at 45GR97 today for the Columbian ground squirrel, particularly during the summer months when about 100 mm of additional precipitation would be needed to make the site area suitable habitat for this taxon today. Even the winter months at the site are too dry for this ground squirrel. This taxon today prefers relatively mesic habitats, usually in grasslands because the Columbian ground squirrel is a grazer (Tyser and Moermond 1983). This species is never far from water, whether free-flowing or in the form of moist sub-surface sediments (Turner 1972). In fact, seasonal sediment moisture, because it determines the vegetation cover, may be a factor which limits aboveground activity and dictates summer aestivation periods of this taxon (Turner 1972). The climatograph thus matches and corroborates the inferences derived from palynological data that central Washington was moister during the early Holocene than at present.

The significance of the climatographs for understanding prehistoric human land use is reflected in a Holocene sequence of cultural adaptations postulated by Galm et al. (1981). They suggest that between 11,000 and 8000 B.P. people hunted large and small

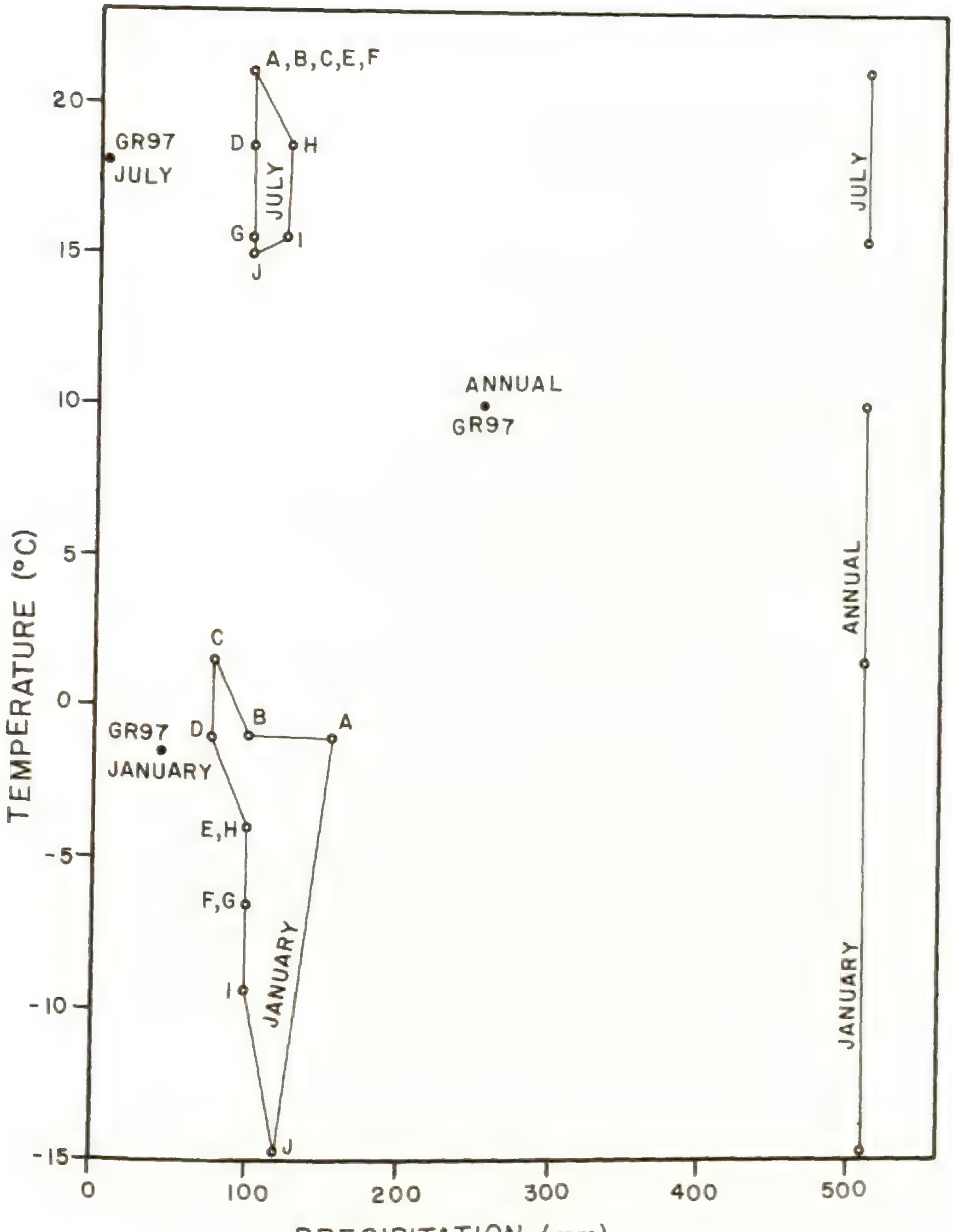

FIG. 5.—Climatograph for Columbian ground squirrel *(Spermophilus columbianus)* derived from data in Table 4. The open circles with letter labels are the geographic points (Fig. 3). The graphs on the left represent the July precipitation plotted against July temperature, the January precipitation plotted against January temperature, and the dots represent the July and January climate at 45GR97 today. The lines on the right represent the July, Annual, and January temperature ranges for all geographic points plotted against annual precipitation (508 mm) for all geographic points. The dot in the center represents the annual climate for 45GR97.

mammals, fished for non-salmonids, and followed a nomadic but seasonally scheduled pattern of movements between winter camps located in the major river canyons and summer camps and resource extraction loci in the uplands. With warming and drying of environments between 8000 and 4500 B.P., people intensified fishing activities and the exploitation of seeds and roots, and spent more time in river canyons and less time in the increasingly xeric uplands than previously (see also Chatters 1982). In Binford (1980) and Kelly's (1983) terms, it appears that a shift from a residential mobility strategy to a logistical mobility strategy took place as environments became progressively warmer and drier. The typical explanation for such a shift in land use strategies concerns changes in resource availability, especially in this case expressed as decreases in (animal) biomass (e.g., Kelly 1983 and references therein). The faunal data on which the climatographs are based tend to conform to this kind of explanation: the disappearance of arctic foxes and Columbian ground squirrels from increasingly xeric habitats in eastern Washington represents a decrease in exploitable faunal biomass in those habitats.

SUMMARY AND CONCLUSIONS

The increasingly numerous discussions of quantitative methods in zooarchaeology present a bewildering set of alternatives and opinions on how to best quantify a sample of faunal remains. Comparable discussions of taxa as attributes of a fauna and techniques for analyzing and deriving inferences from species lists are, in contrast, rare. Perhaps this is due to the relative parsimony of deriving inferences based on nominal scale faunal data and/or the suspected greater potential resolution afforded by ordinal and interval scale faunal data. Regardless of the reasons for the paucity of literature on nominal scale faunal data (a large portion of that literature is found in paleontological journals; see for example Raup and Crick 1979 and references therein), such data are potentially useful and informative. Using examples from the late Pleistocene and early Holocene of eastern Washington, I have shown here that nominal scale faunal data may (1) clarify the zoographic history of a taxon, (2) indicate fruitful analytical pathways towards modeling human subsistence by providing information on the taxonomic composition of faunas of particular time periods, (3) provide data indicative of past climatic conditions, and (4) suggest causes of changes in human land use practices.

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ETHNOBIOLOGY, COGNITION AND THE STRUCTURE OF PREHENSION: SOME GENERAL THEORETICAL NOTES

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ABSTRACT.—Over the last decade taxonomy has been shown increasingly to be an inadequate representation of the results and devices of classifying activity in folk biology. Its concepts of rank, level and contrast, and assumptions about thought processes, do not exhaustively account for observed behavior, while flexibility and contextual considerations have been under-stressed. Taxonomic approaches, moreover, have tended to produce patterns which reflect method as much as anything intrinsic to the data. This paper summarizes some of these difficulties, and suggests that a methodology focused on the notion of *prehension* may provide the basis for a broader approach encompassing both cognitive and social processes involved in the generation of classifications.

> Man and life and nature are none of them domains that present themselves to the curiosity of knowledge spontaneously and passively. [Foucault 1970:72]

In its strict and technical sense, taxonomy is an hierarchical metaphor involving linked notions of rank, level and contrast. In European thought, it finds its first known historical expression in Aristotle, but has since passed via Linnaeus into modern biological usage. As a model of classification it had gained such wide currency by the middle of the present century that a great many ethnographers assumed that it must also necessarily order the folk schemes they were beginning to describe and analyze. This assumption has given rise to the formal theories associated most closely with Brent Berlin and the American school of ethnosemantics (see Ellen 1979b:12-13). In the last decade these theories have come under sustained attack.

The objections have been various and interconnected (eg. Ellen 1979a, 1979b; Fox 1975:118-119; Friedberg 1968, 1970; Healey 1978-79; Hunn 1976, 1977b, 1982—despite Hunn 1975). However, it is helpful to present them under a limited number of headings, even at the risk of appearing to conflate certain matters and separate others that are clearly linked. The headings are: definition, rank and level, contrast, flexibility, context, taxonomizing as a thought process, taxonomic artifacts and taxonomy as theory. In this paper I wish, first, to provide a resume of the various criticisms and then, in the light of my own analysis of Nuaulu ethnozoology (Ellen 1972, 1975; 1976a; Ellen, Stimson and Menzies 1976a, 1976b; Ellen 1978, 1979a, 1985 forthcoming, in press), to outline a broader alternative approach focusing on a concept of *prehension*.

AGAINST A GENERAL TAXONOMIC THEORY OF CATEGORIZATION

1. *Definition.* It has not helped matters greatly that there has been some confusion over what, precisely, is meant by "taxonomy." For some it is no more than a synonym for classification, and in this respect folk taxonomies are seen as equivalent to folk classifications. Thus, Berlin, Breedlove, and Raven (1973:214) use *taxa* to refer to all "linguistically recognized groupings of organisms of varying degrees of inclusiveness." The confusion is compounded by the different meanings attached to the term and its cognates in different European languages. In French, ***taxonomie*** is broadly equivalent to the narrow definition of "taxonomy" in English; while ***taxinomie*** (from the Greek

taxis) has a much broader reference. As Claudine Friedberg (1968:315) points out, while the distinguished nineteenth century botanist, A. P. de Candolle, used ***taxonomie***, Lévi-Strauss uses ***taxinomie.*** It is, of course, possible that this distinction is responsible for the different usages of English language writers. Here I have employed "taxonomy" only in its strict Aristotelian sense. That is, in the sense understood by Kay (1971), as a model which owes its form to the Linnaean analogy expressed in set theoretic terms.

2. *Rank and level.* The notion of hierarchy is integral to the taxonomic model found in ethnobiology. Berlin, Breedlove, and Raven (1973:214) pursue the logic further and suggest that taxa of differing degrees of inclusiveness can be placed in a limited number of ranked categorical types (unique beginner, life-form, generic, specific, varietal) and that these characteristically occur at the same taxonomic level. Upon these distinctions and definitions they erect a complex theoretical edifice which is then used to describe particular ethnographic cases.

The notion of hierarchy, at least as it has been applied to entire classifications, and the insistence on assigning categories to different levels, has sometimes given rise to problems which are altogether spurious. The concept of "level," except in a limited and rather crude way, is very difficult to demonstrate beyond particular local regions of classificatory space in particular domains of particular peoples. Similarly, although, used in a loose metaphorical way, all folk classifications are "hierarchic," none are of great depth in any absolute sense (Hunn 1976:509). If we wish to retain some semblance of taxonomic organization we might preferably do so by opting for the tree-diagram model rather than that of a true taxonomy. That is, not by making *a priori* assumptions as to level. Thus in Fig. 1, although it may well be possible to demonstrate "local" contrasts

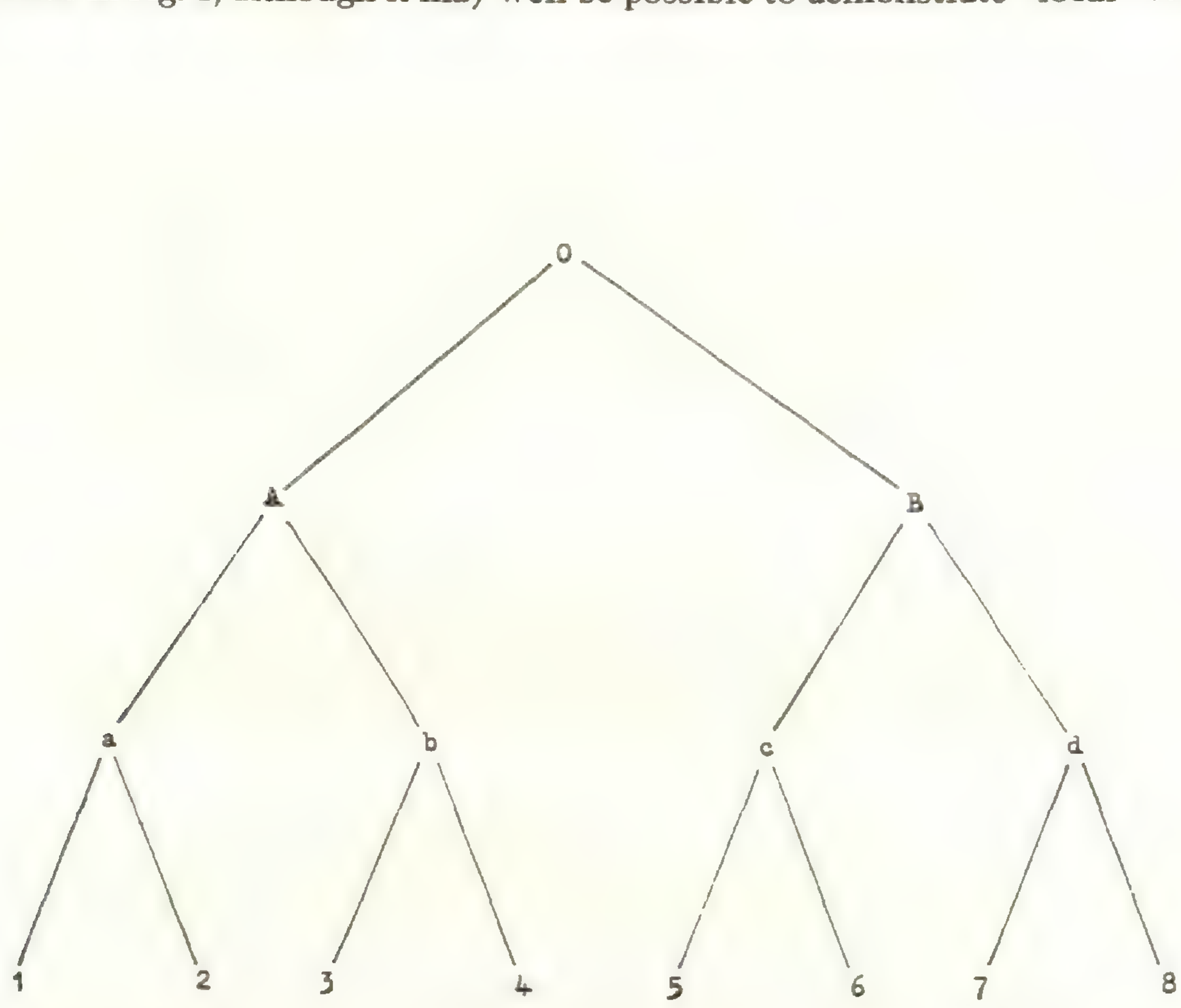

FIG. 1.—Standard abstract representation of a taxonomy. Upper case letter, lower-case letters and figures respectively indicate items at the same *level*. Items at the same level are regarded as being in a relationship of *contrast*. Items at each subordinate level are contrasting *segregates* of a *more-inclusive class* at the next highest level.

(1:2), we cannot always show with the same ease that 1 contrasts laterally with 8 even though 1 and 8 may share the same number of categorical links from 0, as in: 0Bd8, 0Aa1.

The recognition of taxonomic rank requires "inelegant complications of formal taxonomic models" (Hunn 1976:510), provides no basis for either distinguishing induction from deduction, for explaining how taxa or other categories are actually generated, for handling non-transitivity, or for the fact that paired and contrasted items do not necessarily imply more inclusive categories (Brown 1979:794-795; Lancy and Strathern 1981:78). I summarize some of the reasons why this should be so in the remainder of this section.

Hierarchy is not the only metaphor that can express inclusivity in the relations between categories, and there are many ethnographic demonstrations of this. Notions of "broadness" and "narrowness" may, for example, be more appropriate. Moreover, there appears to be a curious notion that classificatory space is consistent with the dimensions of a type of two-dimensional graphic representation which, historically and ethnographically, is of limited extent. Indigenous conceptual arrangements and their linguistic expression may be least violated by employing other means of graphic representation: networks of focal points around which categories cluster, Venn diagrams in two or three dimensions, "sphere of influence" models, or "type-token representations (Bright and Bright 1965; Ellen 1979a:354-357, 1979b:12-14; Friedberg 1970; Tyler 1978:278-279). Hunn (1976:515) has employed the notion of a system of differences in "classification space." Distances within this space are assessed in terms of overall similarity and difference between organisms with respect to perceptible attributes of morphology and behavior. Categories are defined by reference to patterns perceived within this system of differences.

3. *Contrast*. The model of taxonomy employed by Berlin, Breedlove and Raven (1973:214) requires that categories at the same level be mutually exclusive and contrasting. This ethnosystematicist notion developed in conjunction with componential analysis, which has proved inadequate for defining such concepts (Hunn 1976:509; cf. Turner 1974:16-17). Folk biological categories in general are not defined by reference to verbalizable feature contrasts, let alone single characters. They are semantic primitives, at their lower levels generated by induction (Hunn 1975:313, 1976:515). In some classifications not even the notion of contrast need be present, at least not in the sense that we normally understand it. Thus, rather than something being x or y it is common for it to be more x than y, or more y than x. The idea of gradation eliminates distinct boundaries and has been termed by Lakoff (1972) "fuzziness." Thus we are invited to speak of "shrubness" or "treeness," "birdness" or "snakeness" (Randall 1976:549-551). The vagueness can sometimes exceed even such fuzzy expressions as these, especially in the form of such hedges as "well, it might be," "it's a sort of bird" (where the stress is on "sort" rather than "bird"), and "may be." All of this fits well with the idea that the prevailing relations between categories are through polythesis.[1]

4. *Flexibility*. The problem of applying notions of hierarchy and contrast consistently lead us to suggest that taxonomies (and particularly those which are imputed to rest predominantly upon morphological distinctions) have often been assumed incorrectly to be the only (or if not the only then certainly the primary or dominant) means of classification. Berlin's approach has been strongly and widely criticized for attempting to impose a form of taxonomic rigidity on a cultural apparatus the general characteristics of which are quite antithetical: namely fludity, flexibility and elasticity (Bulmer 1974:24; Dwyer 1976:442; Ellen 1979b; Healey 1978-1979). For Friedberg (1974:327), there are a multiplicity of systems of reference, and there is always the possibility of disjunction between the separate spheres of nomenclature, identification and classification; or we

might say between "models for" and "models of," or between keys and classifications. It would appear then that taxonomies have to be extracted and created from a much more complex classificatory web of which they are part, and in the process even constructed in the interests of neat presentation. The evidence for the inadequacy of taxonomy has come to light partly through the so-called "special problems," which many have seen as simply the artifacts of method. Examples of these include multiple and inter-locking hierarchies, the employment of radically different principles at different levels, and extra-hierarchic relations (eg. Conklin 1969:50; Perchonock and Werner 1969:232, 234). Thus, to say that a taxonomy has cross-cutting classes is to beg the primacy of the taxonomy itself, especially when such categories are highly significant in cultural terms, however we may agree to measure this. Synonymy, homonymy, polysemy and anomaly, all lead us to question the legitimacy of an approach in which there are distinct ranked levels and clearly-bounded contrasting categories. Covert categories, in so far as they can be demonstrated to exist at all, seem to contradict the very idea of taxonomy (Taylor 1980:273), as does the fact that taxonomic categories appear in many cases not to partition classificatory space exhaustively, leaving "empty" and unlabeled residual regions (Hunn 1976:57-58; cf. Kay 1971:878). And all of this is not simply a matter of empty theoretical or methodological assertion, but arises from the direct experience of ethnographers attempting to represent the actual means by which real people set about employing categories and labels, and the relations between them, in their everyday lives. I have tried to show this myself for the Nuaulu of Seram, eastern Indonesia. Rather than explaining away such complications as "special problems" (peripheralizing them, making them exceptions to the rule), I am inclined to regard them as central and inseparable devices of classificatory thought. In particular, there appear to be considerable options in folk classifying behavior in terms of the degree of elaboration at internal degrees of inclusiveness (Bulmer 1967:24; Healey 1978-1979:362; Majnep and Bulmer 1977:48). The evidence for conflicting arrangements, cross-cutting ties, and so on, is now so overwhelming that it cannot be ignored in any general theory (Bulmer 1972-1973, 1975; Dwyer 1976; Healey 1978-1979:364).

5. *Context.* Trenchant also has been the criticism of taxonomic and allied ethnobiological studies for becoming divorced from the situational considerations of ethnography, of the context in which folk classifying takes place (Ellen 1979b; Martin 1975). Thus, for Friedberg (1974:320), "une classification d'objets naturels n'est indépendante de ces derniers; ils existent en dehors de la perception que l'on peut avoir d'eux dans une culture particulière." The problem with decontextualization is not simply that it isolates classifications from the rest of culture and thus presents us with something which is abstract, but that by shedding extraneous cultural information it presents us with the illusion that knowledge consists merely in understanding resemblances (cf. Foucault 1970:111). In other words, it is a complete reversal of the ethnoscience position in which an adequate description should provide enough information to know how to perform in a culturally acceptable manner. If the decision is made to isolate classifications as formal systems, treating each as a separate universe of discourse after the fashion of a mathematics which can be used for any description, we reduce classification to the realm of technical procedures, and taxonomies can easily be shown to be adequate descriptions of that reality. The elimination of context enables the assumption that what is being examined is, in fact, a formal *system*. Identifying a taxonomic, or any other classificatory system, is just like identifying a religious "system" in a tribal society; it is always possible to isolate it if you want to, but to do so may lead to a complete misunderstanding of its structure, function and position in the social and cultural fabric. Social and cultural anthropologists, of all people, cannot make this assumption (Harrison 1970, 1977).

6. *Taxonomizing as a thought process.* There has been an assumption in much formalistic research that the reconstruction of taxonomic hierarchies provides us with a basis for understanding thought processes involved in classifying behavior, that categories are classified in a particular way as a reflection of how people think. It is now clear that taxonomic classifications play a lesser role in human thinking than has been hitherto assumed, especially outside the area of biological kinds (Wierzbicka 1984:325). Strictly taxonomic categories have often not been distinguished from other types of category, the conceptual relation "kind of" has not been clearly dissected from the referential relation of set inclusion (Wierzbicka 1984:313, 315). But even in ethnobiology there has been considerable scepticism. Randall (1976) has criticized the taxoomic model on the grounds that its assumptions about transitivity suggest incorrectly that elicited taxonomies represent structures involved in memory storage. Rather, he suggests, taxonomic trees are the *result* of classifying behavior, and not the means by which information is stored. It is also now clear that certain hierarchical relations are not transitive, and that complex hierarchies are often a product of the procedures used. Connections between different categories are regularly made using short-cuts, in ways which seem to violate taxonomic reconstructions. Thus, in Fig. 1, the relationship between 1 and, say 6, is commonly not arrived at via the route *aABd* (as would be necessary using taxonomic logic), but often directly or through another level category. We may therefore conclude with Hunn (1977a:12) that, "it is more valid psychologically to describe the classificatory structure as based on non-hierarchical relations of perceived similarity." This is why network and spatial models are somehow more attractive than the undirectional, duodimensional taxonomy. Moreover, as Hunn has pointed out, "taxonomic theory provides no basis for distinguishing induction from deduction in logical thought." He argues that more inclusive categories, such as "birdoid," are determined deductively, while less inclusive categories, such as "sparrow," are determined inductivly (Hunn 1976:510, 519). The evidence for this now seems overwhelming.

7. *Producing taxonomic artifacts.* If folk taxonomies do not reflect the actual psychological processes involved in many of those activities we describe as classifying, it is necessary to ask what it is they do reflect. They were at first thought to reflect a true emic model. This, after all, was the aim of the "new ethnography" of which folk biology has been a noteworthy part. It does, however, begin to look more like an etic model of the emic, and then of a very particular kind, where the analyst has already decided what the model should contain and selected results accordingly, even rejecting some statements on the grounds that they appear to represent the idiosyncratic interests of particular informants.

What they do reflect, of course, to a very considerable extent, is an observer's model of taxonomy, which in its most "tight" stereotypical Linnaean form has been outlined by Kay (1971). The attraction of such a model lies partly in its intrinsic aesthetic appeal, partly in a desire for parsimony, lucidity and rational order, partly in its demonstrable resemblance to some processes in folk classifying, but partly also in its implicit, and occasionally explicit, mimicry of scientific natural historical taxonomy and linguistics. This latter influence is evident both in terms of how the enterprise is phrased, the ways in which data are conceived, in the technical procedures for elicitation, in the formal precision of analysis and in the formulation of the problematic. In some cases, I would suggest that individuals with a grounding in natural history have a hidden bias towards finding "natural" categories, and towards an under-emphasis of variation, with a corresponding stress on the taxonomic approach. One gets the impression that ethnobiological taxonomists are by inclination collectors, and as Bulmer (1974:82) has pertinently remarked:

> . . . almost all collectors like nice, perfect specimens, and derive considerable aesthetic pleasure from the ordering *they* impose on them. A danger which the

> ethnobiologist must guard against very consciously is that of letting his personal aesthetic judgements override his data . . .

But more generally, while the specific scientific model is certainly the immediate and most obvious source for representations of folk models, it is well to remember that the taxonomic model and its close classificatory congeners is grounded in a special cultural tradition, in which the graphic and written representation of the relations between categories markedly alters, rigidifies and directs their conceptualization (Ellen 1979b).

In addition to the specific conventions of literacy, graphic representation and the scientific tradition, there is the general fact that in all inter-cultural communication (of which ethnobiological ethnography is just one, very specialized, example) all local explanations have to be rendered in a form which is in some sense meaningful to the ethnographer. Although we pride ourselves on our grasp of alternative worldviews and organizing principles, we cannot be sure that we ever known everything that is relevant, since knowledge may be caste in an idiom with which we are quite unfamiliar, and therefore ill-placed to understand. The history of anthropological theory itself provides ample confirmation of this, as successive explanatory frameworks are able to indicate previously unexpected relationships and arrangements in old data. Moreover, within the confines of our own range of conceptualization we are always predisposed (by virtue of our cultural socialization and professional training) to favor one view rather than another.

The attraction of the taxonomic model is that once you have data devoid of contextual considerations (in their widest sense) it is virtually impossible not to put a taxonomic construction upon them. It is easy enough to represent a classification as taxonomically ordered and based entirely on morphological criteria if you *a priori* assert that this is what you are looking for. If you are attempting to reconstruct a native conceptual universe as it applies to plants and animals then you cannot begin by excluding categories and arrangements based on non-morphological criteria since informants do not, in the coursee of their ordinary lives, necessarily make such discriminations. In my experience people do not regularly make judgements which suggest that they operate with an all-purpose classification which is recognized as being in any way separate from, or different to, classifications organized in some other way.

As soon as you begin to exclude certain categories and arrangements from consideration then, of course, you begin to yield regularities which look much more like contrasting, hierarchically-ordered and ranked taxa. Thus, the unambiguous all-purpose, morphologically-based taxonomy is something which the ethnographer or linguist *extracts*, but even then seldom perfectly. What is extracted may serve as a basis for establishing the existence of universal processes, schemes of categories, but equally it may do no more than reflect an artifact created by common techniques of extraction and representation. What is more, the very character of taxonomizing as a process generates anomalies.

To summarize: many of the principal data-production techniques employed in cognitive anthropology and ethnoscience tend to provide us with taxonomies and their associated anomalies (see also Gardner 1976). Formalized question and answer frames to a large extent determine the outcome (Frake 1980 [1977]:49), and constrain otherwise articulate informants. Informants, unprompted, rarely in the course of their ordinary lives will use expressions such as "is x a kind of y," or "how many kinds of y are there?" Culturally inappropriate questions, assumptions as to the existence of unitary correct representations, are commonplace. We have all experienced severe frustration in attempting to answer correctly and honestly multiple-answer questions or public opinion poll style survey questions. But in addition to this, techniques such as card or slip-sorting, the drawing of tree diagrams and other techniques which mechanically encode an implicit or explicit assumption to dichotamize successively, will unfailingly

produce formal hierarchies, while eliciting definitions ruthlessly erases any fuzziness between categories as ordinarily used. It is only possible to approach the essential cultural reality of categories if techniques are *for the most part* basically non-directive, that is much looser and more varied (see Perchonock and Werner 1969:236-238). Hence, in my own work I have increasingly relied on simply listening to people talking about animals, using prompts which are less inclined to force the data into formal taxonomies.

I am particularly troubled that some scholars should believe so readily that taxonomies are incontravertibly "in the data," that they emerge "from the data as a consequence of their *natural properties*" (see Lakatos and Musgrave 1970:98). The taxonomic view of classification is in this sense like Lévi-Straussian structuralism, in that if you try hard enough it is possible to discern the kind of order you are seeking, wherever you wish. My field data (by which I understand what was written down in my notebooks, on cards, on specimen tags, in photographs and drawings, and recorded on magnetic tape) were obtained through a mixture of directive and non-directive methods. In this form they consist of fragmented, sometimes contradictory statements, and have to be "processed," transformed into clear generalizations, testable hypotheses and descriptions. There is nothing *in* the data in this form which would suggest that a distinction between morphological and non-morphological characteristics, or between taxonomic and non-taxonomic processes, is justified, or that there exists something called "the Nuaulu classification of animals," which is conceived of as some kind of structured totality. This is a construction which I have placed upon the data. Certainly, one of the properties of the Nuaulu data (as I have presented them) is that many permit a taxonomic construction; but they may permit others as well. The "natural" properties referred to are ambiguous and the term contentious. I would wish strongly to resist the kind of empiricism which uncritically sees taxonomies as simply facts out there waiting to be collected, like so many herbarium specimens. Such an approach begs crucial questions in the understanding of classification, and (ironically) its general application has only been possible through the systematic neglect of the full range of factors at work in "classifying." In the highly particular social world of professional biology the principle of "taxonomic rigidity" is an important working assumption; in the context of ethnobiology it has become simply dogma (Healey 1978-1979:379). But you cannot work from taxonomy as if it provided a set of axioms (Hunn 1976:510).

8. *Taxonomy as theoretical icon.* A final problem, and perhaps a factor explaining the tenacity with which its practitioners defend the taxonomic approach, is linked to the fact that it is not simply that taxonomy and formal elicitation has become indelibly linked to certain kinds of substantive investigations, but that "taxonomy" has become central to, even to emblazon, particular theories of culture (Conklin 1969), conceptions of ethnography (Spradley 1979), and models of thought processes (Bruner, Goodnow and Austin 1956). In other words, it has become reified; to some extent at least because it provides a universal model to counter cultural relativism (Brown, Kolar, Torrey, Truong-Quang and Volkman 1976). Curiously (and paradoxically), the approach is based on the rigorous definition of cultural boundaries in order to provide the basic units for the construction of pan-human hypotheses. However, this notion of the boundedness of cultures is linked to an organic model and crude functionalist assumptions, without being genuinely sociological, systematic or contextual. This becomes readily apparent if it is compared with the treatment of classification in, say, historical linguistics and philology. In the latter, cultural boundaries disintegrate, and diffusion and historical explanation rule, and the patterns themselves *are* contingent upon particular social and cultural processes.

BESIDES TAXONOMIES

While we may agree that attempts to define, rigorously, the principles of classification and nomenclature in folk biology are certainly useful for particular ethnographic populations, the making of inductive generalizations about certain types cross-culturally assumes that variability is according to a limited number of well-understood criteria along parallel axes. The restricted check-list approach exemplified by the work of Berlin and his associates cannot, then, cope with the wider dimensions of variation between systems. It not only tends to reify a particular kind of classification (that which we call taxonomic), but seems to claim that a large number of semantic fields are at all times similarly organized. "Taxonomy has been elevated to an artificially high status as *the* mode that humans employ to organize and act upon discrete elements in the environment" (Rosch, Mervis, Gray, Johnson and Boyes-Braem 1976). It is compelling because it is a stylish (p. 790) representation of relationships among natural elements and because taxonomizing appears to us as an efficient strategy for organizing, storing, and retrieving elements (especially words) in memory (see eg. Ericsson, Chase and Faloon 1980). Even if we agree that the taxonomic mode typifies Western culture (and I am not convinced of this either), we cannot assume that all cultures have exactly the same formulations of resemblance, relationship, class or contrast (see Hobart 1982:56). Nevertheless, many field researchers, sceptical of the claims for an all-embracing taxonomy in human categorization of nature, have reasonably argued in favor of a "limited natural taxonomy," or have found taxonomies a convenient descriptive framework (Ellen, Stimson and Menzies 1976b; Taylor 1980:285).

We may agree that, as one available common process, taxonomy is universally available in the classifying repertoires of all people. It is, however, more important in some societies than in others (see also Super, Harkness and Baldwin 1977), and although the taxonomic mode is useful in describing the structure of some systems it is not entirely adequate (Hunn 1977a:13), and in some cases limited to particular domains (Lancy and Strathern 1981:780). Thus, taxonomy works quite well for Nuaulu vertebrates (and among vertebrates for birds and reptiles), but it works less well with mammals, invertebrates and fungi. Elsewhere, although the Tobelorese use non-taxonomic features they appear to prefer taxonomic structure, especially "below" basic terms (Taylor 1980:276-277).

Using procedures for testing taxonomic thought (see Bruner, Olver and Greenfield 1966), and using tests to reflect presence or absence of taxonomic thought, it has been suggested that there is a close correlation between the tendency of children to employ taxonomic-like strategies and the degree of depth and complexity of folk taxonomy in a language (Lancy and Strathern 1981:774). Work undertaken by Lancy and Strathern in two New Guinea populations suggested that Ponam children improved their taxonomic thought with age whereas Melpa only improved with education (p. 777). "Melpa lessons are in English and advances in learning names of things (as opposed to categorizing) in mother tongue should be associated with general improvement in fluency" (p. 778). Melpa appear to mute a taxonomizing tendency as socialization advances (p. 778) and other modes of representation are employed; in tests pairing was interpreted as blocking taxonomizing. In the case of the European tradition, the taxonomic approach is firmly linked to the development of literacy and scientific culture. So, where literacy has even just a toehold (as among the Tzeltal) it cannot but help encourage taxonomic expression.

Thus, two things become abundantly clear: firstly, that the cognitive and linguistic constructs employed in classifying are varied and combined in different ways in different cultures, and, secondly, that the ways in which they are employed are exceedingly flexible. In addition to hierarchic class inclusion, folk biological classifying activity involves indices, keys, paradigms, typologies (Conklin 1964:39-40), non-hierarchic binary

opposition and pairing (Brown 1979:794-795; Lancy and Strathern 1981:782), simple dichotamous division, and possibly other forms as well (Tyler 1978:290). We must also be prepared for categories to be expressed in different ways on different occasions in different places. Thus, pairing may involve a wide variety of principles and these will be realized differently in different cultures: difference, duality, complementarity, alliance, hostility, equality (Lancy and Strathern 1981:788). Similarly, pairs may be elaborated to form more complex constructs, analogies, series of paired opposites, series of vertical similarity, alternation, and more complex symmetries. These basic relations in ordering social classifications are well-known (Needham 1979).

Moreover, items may be assigned to different categories, arranged in different ways, according to different principles, depending on context. In other words, culture *enables* various forms of alternative orientations, organizations and actions; culture in this sense is a tool-kit (Salzman 1981). Friedberg (1971) has shown this for Bunaq plant categories. For the Nuaulu, it occurs not only in terms of the allocation of certain salient terminal categories to more inclusive ones (Ellen 1975), but also in the identification of the content of terminal categories themselves (Ellen, forthcoming). How these different modes are employed will vary. Taxonomy will, in certain cultures, be dominant. In some cultures styles of classification may vary between domains, which may have their own special organizational structure; elsewhere they may vary according to situation.

UNDERSTANDING THE GROUNDS FOR PREHENSION

Rather than documenting taxonomies or other kinds of classifications and categories as so many butterflies (Leach 1961:2), it is necessary to focus upon the processes which generate them; not detached cognitive processes, but those rooted in particular situations. To distinguish it from the arid abstraction of the notion of classification, we might call this *prehension*. Prehension refers to those processes which through various cultural and other constraints give rise to particular classifications, designations and representations. What results depends on the input at all stages in the process (elicitory techniques, etc.) and the interaction of various factors. Prehension stresses the situational bias of classification, whereas *cognition* and *perception* suggest purely cerebral processes. Indeed, classification itself may be deemed too narrow a definition of what is involved (Reason 1979), and too easily ends in psychological reductionism, in a discussion of states of mind. Prehension recognizes, without the necessity of qualification, the difficulty of distinguishing mind from matter, thinking from doing or speaking, individual from group, cerebral from social, natural from cultural. Thus, prehension entails individual acts of perception, but is not (and cannot be) confined to them. We can only begin to approach a realistic understanding of classifying behavior if we begin by observing people assigning items to categories and using names in natural ethnographic settings, as well as experimental ones. Moreover, if we use experimental techniques which we might reasonably expect to produce particular results, we should try also to devise other techniques which might produce other, different, results.

The structure of prehension is as follows. People bring to situations in which classifying activity takes place, and from which verbal statements about classifying behavior result, information of diverse kinds acquired through both informal and formal socialization experiences of the world in general and of earlier classifying situations. How they then classify depends upon the interplay of this past knowledge (including prescriptions and preferences with regard to particular cognitive and linguistic idioms) with the material constraints of the classifying situation, between conscious and subconscious, the purposes of the classifying act, and the inputs of others. Thus, thinking, saying and doing are not separate activities but inter-penetrating ones, while cognitive bricolage provides us with both models "of" and models "for" (in terms of

Geertz's distinction (Geertz 1966).[2] Practical problems do not exist on their own, and in a very real sense all classifications are, therefore, "practical" (see also Hunn 1982).

There is a further important aspect of prehension. This arises from the fact that the processing and storage of information in the mind is imperfect, and communication of that information less perfect still. Paradoxically, there is a connection between this shortcoming and the considerable capacity of the human mind to re-order information in different ways, replacing irrelevant information with that of greater and more immediate utility. That classifications are messy, cross-cutting and changing is a reflection of this. Consider also the paradox that while the human mind always strives for order, the reality it deals with is so complex that it can never fully attain it. Concepts are often used, operationalized, without defining them. On the other hand, however, in order for communication to take place, classification must have at least some intersubjective structure, some agreed cultural rules, some "doxa" (Bourdieu 1977).

Prehension is an inherently social process. Classifying activity may be solitary or inter-personal; that is we may wish to communicate not simply with others but with ourselves as well. But even solitary behavior is modeled on that hypothetically occuring between individuals. In a solitary classifying act there is no communication with others, but, nevertheless, thought usually takes the form of linguistic expression, though not necessarily verbalized. The problem is that at the point of transformation into the lexical output of language there is a fundamental simplification of cognitive and semantic relations. Linguistic expression necessarily entails both *decomposition* and *facilitation*; decomposition because language faculty is unable to encode at one instance the totality of informational relations in the mind, and facilitation because that same complexity must be translated into a form which makes communication and expression possible. It is, if you like, the same as translating from the machine code of a computer, designed for the internal organization and manipulation of information, to a programming language. Thus, it is impossible for a sender to communicate everything to a receiver. We must, therefore, distinguish the intended message from the outward signs of the message and it is these latter which will vary depending on who the recipient is. Receivers will encode the message, not in terms of the intentions of the sender, but in the light of the recipient's own expectations and knowledge. Sender A may say "X is a kind of Y," based on the unverbalized information that X and Z are types of Y, although recipient B (with no knowledge of the Y-Z relation) may interpret this statement, through his or her own experience as a fourth item W, to imply that X (like W) is a kind of Y. There is, then, a degre of ambiguity, and people must interpret and operate with respect to the codes and outward signs of others without knowing what inner processes are taking place and the information which generates them (Wallace 1970). We may, therefore, agree (with Reason, n.d.) that "linguistic utterance is not, generically precise at all; it is generically *sufficiently* precise." Interpretation depends upon whether the person of the same culture interacting in a particular activity will be different from that of a member of the same culture but one who is not prepared by previous mutual experience. The interpretation of a non-member of the culture will be different yet again, and that of the specialist ethnographer especially so. All this will affect the degree of possible ambiguity. Interpretation will also depend upon the questions or commands of the interlocutor (recipient). For example, an individual may get from A to B according to a variety of ad hoc conscious and unconscious procedures, and certainly without the use of any mental map. On the other hand, if asked to draw one *post facto* there may be no problem, even though the map had no bearing on the original decision. As Reason (Reason, n.d.:7-8)(see also Crick 1976:159) has pointed out: "ambiguity, ambivalence, metaphorization, are not peripheral and arcane aspects of language use, but central and essential . . . It is hypostatized, reified classificatory usages which require special social conditions to obtain." What this implies, and what I have tried to focus on here, is

classifying (as an historically situated activity) rather than an emphasis on classifications. "Classifications," says Reason in continuing the passage just quoted, "as such are, if at all, only derivatively meaningful." Indeed, formal representations may be "positively misleading, for they purport to, but cannot incorporate the grounds of such interpretations" (cf. Tyler 1978:290).

THE COGNITIVE ACQUISITION OF THE INSTRUMENTS OF PREHENSION

The dynamics of prehension cannot simply be understood in terms of the interplay of factors at the instance of classification or verbal expression. The outcome depends on the life experiences of the classifier: learned cultural behavior, personal experience, adaptation and individual socialization. In this respect it is important to acknowledge the significance of materiality and the child's acquisition of that materiality. Thus, in the early development of every child, bodily discovery, experience and perceptual salience will determine a cultural dominance of *front* over *rear*, *above* over *below*, *hands* over *feet*, and so on (Clark 1973). Linguistically, the second item in each of these pairs will be accordingly marked. But not only does materiality affect the handling of knowledge, but the experience of time also. Thus, the *past* appears to markedly dominate the *future*. This must be is so in two senses. First, past experience has cognitive priority and is only displaced through repeated contrary cases. Secondly, temporal ordering itself serves as a basis for serial signification (as in the contrast set *older:younger*, or in birth order names).[3] Thus, overall, the dominant cognitive relations brought to prehension are material, historical and biographical, rather than non-material and contemporary.

In addition to such processes ingrained in early socialization, we must add the linguistic and classificatory idioms resulting from cultural convention, but what we must not then do is simply to accord to the mind a mechanistic model. The mind itself organizes information extensively in terms of paradigms, and is an active rather than a passive system in which images are connected and constantly transformed. What is certain is that unless there is clear and explicit cultural evidence for a total unitary classification of animals, it is as unlikely to be generated subconsciously in the mind as it is to be a logical consequence of the structure of language. For Ardener (1980) "it will be no wonder if we cannot sometimes tease out in real life whether we are dealing with a "social" or a "linguistic" phenomenon. Language is to the social as a measuring rod is to the measured, where, however, the inches or centimetres stretch or contract at the same time as the object itself deforms in related or independent directions."

SEMANTIC UNIVERSALS AND CLASSIFICATIONS IN SOCIETY

Once we have understood the process of prehension, and the degree of predictability as to its outcome among particular populations, we can return to the level of societal generalization and cross-cultural comparison, and at once the debate between universalists and relativists is seen as the caricature it inevitably must be; an entirely false opposition sustained through ideological mystification and polemic (Ardener 1982:3; see also Ellen 1979b; Hollis and Lukes 1982 [particularly Gellner]). Classifying behavior *does* reflect social organization, but the degree to which we can discover close correlations will depend upon the constancy in application of a particular mode. One possibility is the employment of grid-group forms of analysis pioneered by Douglas, 1982. I have elsewhere (Ellen 1979b) attempted to list the mix of variables for any given society that is likely to affect the structure, content and function of classification. It is possible to demonstrate correlations between pairs of characteristics: between division of labor and classificatory complexity, literacy and arbitrariness, semantic field integration and social

integration, and so on; linking the formal properties of particular classifications with the substantive ones of the societies in which they are found.[4] If we could eliminate all other variable we might reasonably expect such *horizontal* pairs to show a regular correlation. The problem for the anthropologist, and one reason why I am suspicious of attempts to seek constant macro-relations between classifications and types of society defined in terms of vague and general criteria, is that it is not always possible to find predictable regularities in the *vertical* relations between variables. For example, literacy does not always accompany hierarchy, while rigorous expression of inclusiveness may eliminate anomaly as well as generate it. The pattern observed and the extent to which particular pairs of correlation are evident depends upon the entire nexus of variables.

The attempt to generate a neat concordance demands care, since for one thing it seeks cultural generalizations on the basis of very limited information about the behavior of individuals. Since, as I have indicated, the process of prehension operates through individuals in the context of collective social experiences, it can hardly be expected to coincide with statistical generalizations or necessarily reflect what is culturally dominant. I detect a confusion of the individual with the collective level in work in the taxonomic tradition, in which culture is assumed to be some mythical omnipresent speaker-hearer, both the sum of its component individuals and a constant from which we might infer the classifying behavior of individuals.

On the other hand, we cannot deny that universals can be extracted, though their character must be subject to considerable qualification. Brown's universalist-evolutionary arguments (eg. Brown 1984) have, for example, been severely criticized, along with his presumption of a definitive, monothetic set of "life-form" categories specified in terms of content and structure which exclude non-morphological groups. Such categories are polythetic, structurally diverse, and much more likely to involve special purpose significata; as categories become more general so they become more cultural, less biological (Ellen 1977; Hunn 1982:12-16; Randall and Hunn 1984). Similarly, the detailed wiring for some aspects of color classification sheds little light on how basic naming principles shape language, and it seems unlikely that detailed neural specifications will find much of a place in explanations of language universals. However, it is possible to detect apparently universal ordering principles underlying the character of lexica: conjunctivity (including binary opposition), criteria clustering, marking, and dimension salience (Witkowski and Brown 1978:443-444). It appears that a "rich cognition" model, one permitting both the intrusion of general underlying principles and possibly domain-specific ones, is warranted by the little evidence available. But while such a model is attractive, our attempts to tease out convincing domain-specific semantic universals, other than for color, have not yet met with much success. So, rather than stressing the patent substantive invariance of semantic universals through formalism, it is equally important to stress isomorphic patterns, that is the latent relational aspect (Bateson 1973:615; Lévi-Strauss 1966). It is this, rather than the former, which accounts for the feasibility of cross-cultural communication, and the substantive semantic continuities which give rise to the very real problem of misunderstanding at the level of close interactions between individuals. This is so much the case, suggests Ardener (1982:4), that it is itself a human universal. Thus, before we can, with confidence, make claims for the existence of semantic and lexical continuities, it is first necessary to consider the *limits* of cultural discontinuity. In other words, the formalists have got it the wrong way round. Rather than making *a priori* essentialist assumptions (wrapped up in positivist methodology and ethology) which limit the appearances we decide to subject to analysis, we should instead follow the practice of Bayle (Flug 1971:5), who in his *Dictionnaire historique et critique* sought universals which encompassed all conceivable appearances, including the most obscure and atypical.

NOTES

[1]One of the most barren attempts to employ notions of hierarchy and contrast with respect to a particular domain must surely be Stark's (Stark 1969) analysis of body parts. In a domain (the body) where classifying procedures are necessarily analytic rather than synthetic owing to material continuity of the parts (see Ellen 1977b), the notion of level becomes absurd and entirely arbitrary. Thus, in what sense does "face" contrast with "knee" at all, and why should it not contrast with "head"?

[2]As representational models are not neutral in their relation to action, it might even be argued that the notion of representation should be avoided altogether. Moreover, Geertz's distinction is a product of a literate tradition in which representational models and plans for action are more obviously separate.

[3]I am grateful to Kevin Durkin for pointing out to me that the dominance of past over future is controversial among developmental psychologists and philosophers of time. Thus, we must contrast a "moving time" view, in which ego is static and time passing by, with a "moving ego" view. There is also some disagreement in the literature as to which of, say, "before" and "after" should be said to be the marked term. Nevertheless, in mundane classificatory events, as in much individual interaction and subsistence decision-making, practical experience (of the past) on (future) action is crucial.

[4]For a specific ethnobiological example see Dwyer, 1979:19, 25.

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TOPICS AND ISSUES IN ETHNOENTOMOLOGY WITH SOME SUGGESTIONS FOR THE DEVELOPMENT OF HYPOTHESIS-GENERATION AND TESTING IN ETHNOBIOLOGY

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ABSTRACT.—This paper defines ethnoentomology, briefly traces the history of the field, surveys the literature in major subject areas and offers suggestions for continued research. Hypothesis-generation/testing is suggested as an important "intellectual bridge" to a world science that builds upon knowledge systems of all human societies. Examples are presented.

INTRODUCTION

Definitions, even for ethnoentomology, are often difficult to formulate, and, once formulated, are usually unsatisfactory. Insight and understanding is sometimes increased through a comparison with a related term or concept, hence the juxtaposition of "cultural entomology" and "ethnoentomology" in the discussion that follows.

Cultural entomology treats the influence of insects upon the "essence of humanity as expressed in the arts and humanities" (Hogue 1980). Cultural anthropologists usually restrict their studies to "advanced," industrialized, and literate societies, maintaining that entomological concerns of "primitive" or "noncivilized" societies are in the domain of ethnoentomology. They are principally interested in written forms of cultural expression and limit their studies to physically recorded sources of literate societies. It is well to note that this, like many divisions, is an artificial one, and it implies an ethnocentric "we/they" bias built upon assumptions of fundamental differences between "primitive" and "civilized" classification and thought. Thus far, anthropological research has not substantiated such assumptions.

Although the prefix "ethno" generally indicates knowledge of "folk" societies and the word cell "ento" refers to insects (thus ethnoentomology is concerned with the knowledge and use of insects in different human societies), defining the term is not as easy as might be expected. A fundamental problem is that of delimiting entomology itself. Even though the concept "insect" is clearly defined by Western science, entomologists also frequently study "related arthropods." Since these two concepts gradually developed in Western science, it cannot be assumed that they are universal and, in folk societies, must be elicited using *emic* procedures that "discover" conceptual paradigms rather than methods that impose preconceived concepts upon the society under study.

There are a number of areas within ethnoentomology which can be successfully researched through analyses based upon observations and data collection using the categories of Western science, ie., using the *etic* approach, without diminishing their ethnoscientific contribution. Examples include studies of insects as food, the role of arthropods in disease transmission, hallucinogenic insects, the use of insects for ornamentation, problems in contamination of food with insects, etc. Few studies have passed

from the *etic* to the cognitive *emic* level. Yet the native (folk) view of insects—their naming, classification, and use—is surely the ultimate goal of ethnoentomology.

This paper gives a general survey of both *emic* and *etic* topics in ethnoentomology, utilizing the general Western concept "insects and related arthropods" as a unifying category for comparative study. Cultural entomology is treated as a subdivision of ethnoentomology that deals with recorded sources in literate societies. Cultural entomological interests will, therefore, be incorporated throughout the paper, although no attempt is made to review the vast literature.

The purpose of this review is to outline areas of interest for future ethnoentomological investigation, with an attempt made to establish ethnoentomology, and ethnobiology in general, as a hypothesis-generating and testing mechanism. That is, to show how folk knowledge and beliefs can serve to generate new ideas and hypotheses which can then be investigated and tested by our own science. This approach provides an intellectual bridge between Western and folk sciences as well as the basis for a non-culturally biased world science. The paper argues that folk specialists must be treated as scientists, with their respective systems regarded as invaluable codifications of human observations of natural phenomena.

A BRIEF HISTORY OF ETHNOENTOMOLOGY

Development of entomology as a folk science has been traced for Egypt (Efflaton 1929), the Middle East (Harpaz 1973), Greece and Rome (Scarborough 1979), and other parts of the world (Essig 1931; Montgomery 1959; Wilson and Boner 1937). Modern entomology acquired a distinctively humanistic flavor (and perhaps its "ethno" tendencies) from entomologist-philosophers such as William Morton Wheeler, Maurice Maeterlinck, and Jean Henri Fabre, who "not only described insect phenomena with imagination and brilliance, but wrote and spoke of their meaning on a human intellectual plane" (Hogue 1980). Contemporary ethnoentomology began in the Nineteenth Century with the works of Wallace (1852), Daoust (1858), Bates (1862), Hagen (1863), Katter (1883), Librecht (1886), Glock (1891), Marshall (1894), and Wagner (1895). Writings by Armbruster (1926), Arndt (1923), Barrett (1925), Caudell (1916), Dammerman (1929), Ealand (1929), Gudger (1925), Knortz (1910), Laufer (1927), and Nordenskiold (1929) brought the subject into the Twentieth Century. Essig's (1934) survey of the importance of insects to the Indians of California established the traditional categories of ethnoentomological interest.

Zinsser's (1935) *Rats, Lice and History* remains a classic because of its perspective of insects as forces in human social and biological history. *Insects as Human Food* (Bodenheimer 1951) likewise brought insects to world attention in a more positive light as a potential and important source of protein. Wyman and Bailey (1952) were the first to use the term ethnoentomology in print in their seminal work on the Navajo Indians.

The writings of Schimitschek (e.g. 1968, 1977) certainly establish him as a major force in cultural and ethnoentomology. Other general works include those by Clausen (1954), Cloudsly-Thompson (1976), Hitchcock (1962), Hogue (1980), Kevan (1974, 1979, 1980), Posey (1976, 1977, 1978, 1979, 1980, 1986), and Ritchie (1979).

Conklin's (1973) general bibliography of folk classification offers an important section of entries on ethnozoology (including ethnoentomology) and provides a bibliographic framework to link ethnoentomology with its theoretical roots in ethnoscience.

INSECTS AND HUMAN HISTORY

Zinsser's (1935) work popularized the knowledge of the association of insects with the spread of epidemic diseases that demolished empires and changed the course of human

history. Subsequent works (Cloudsly-Thompson 1976; Hare 1954; McNeill 1976; Ritchie 1979; Sigerest 1951; Smith 1973) trace the plagues and pestilence caused by insect-borne diseases such as bubonic plague, typhus, yellow fever, and trypanosomiasis. Bushvine (1976) details the effects of ectoparasites on human hygiene and medical history.

Crosby (1972) analyzes the complexities of trans-Atlantic exchanges of insect-transmitted diseases and emphasizes the destructive impact of such on aboriginal populations of the New World. Often such devastation extended well into regions with no direct contact with Europeans. This was due to extensive aboriginal trade routes that brought goods infested with insect vectors deep into the hinterlands (Posey 1976). The complete impact of insect-related diseases is still little known for the Americas (Dobyns 1966).

Certainly the role of insects in human evolutionary history is indisputable. Students interested in this broad area should begin their studies by consulting the bibliographies of the above works.

INSECTS AND HUMAN FOOD

The most extensive literature in any subject of ethnoentomology concerns the relationship between insects and human food. The study of Entomophagy, the direct use of insects as human food, has a long and varied history. *Why Not Eat Insects?* (Holt 1885) stimulated a series of studies concerning the nutritional potential and importance of insects to the human diet. Subsequent general surveys (eg., Bergier 1941; Bodenheimer 1951; Conconi et al 1981; Curran 1939; Dufour 1981; Gorham 1976a,b; Harlan 1976; Hoffman 1947; Meyer-Rochow 1973, 1975, 1976, 1985; Ruddel 1973; Taylor 1975) have investigated the variations in cultural practice of entomophagy. Other studies have documented the biological efficiency of insect reproduction and the consequent production of protein (DeFoliart 1975; Dufour 1981; Meyer-Rochow 1975, 1976). Recent works discuss the practical problems of insect foods for Western societies, including socio-economic factors, manpower, preparation, handling, and marketing (Conconi 1982; Dufour 1981; Gorman 1979; Ramirez et al 1973; Kok 1983).

Insects are also consumed indirectly through the ingestion of contaminated foods. This is because of the impossibility of complete removal of insect parts from food products (Caron 1978). Contamination necessitates the establishment of a complex set of rules and standards utilized by government food- and drug-regulating agencies (Taylor 1975). Detailed works outline the hazards of insect ingestion which include allergic reactions, poisoning, tumorigenic stimulation and related health problems (Choovivathanavanich et al 1970; Dufour 1981; Gorham 1975; Pimental et al 1977; Taylor 1975).

The major factors affecting insect consumption are not health hazards, however, but cultural biases. Bodenheimer's (1951) book on insects as human food stimulated a series of works regarding cultural traditions and taboos of insect eating (eg., Aeschlimann 1982; Catley 1963; Meyer-Rochow 1973, 1978; Ruddel 1973; Taylor 1975; Tihon 1946). Although Western societies have a particularly strong bias against insects as food, with honeybees being the only Arthropod systematically exploited for human food (Dufour 1981), many other societies have a long and extensive inventory of useful and edible species (eg., Aldrich 1921; Catley 1963; Daoust 1858; Tindale 1953; Wallace 1852; Silow 1976, 1983). Techniques for the evaluation of insect nutritional qualities have been developed (Coconi 1977; Coconi et al 1981, 1984; DeFoliart 1975; Tetotia and Miller 1974) and allow for the generation of numerous lists of species and their dietary potentials (eg., Dufour 1981; Redford and Dorea 1984; Taylor 1975). Such techniques are not without problems and refinements in protein and nutrient evaluation are still needed (Redford 1986).

Other indirect effects of insects include the enormous cost of agricultural chemicals used to control them. A dramatic worldwide increase in mechanized monocultural

planting has led to sharp rises in epidemic outbreaks of insect pests and the resultant rise in crop loss (Altieri 1983; Cooper and Tinsley 1978). This trend, combined with soaring energy costs, has created serious global problems and threatened the stability of food prices in both developed and under-developed countries (Altieri 1985). Other farming trends, such as "no-till" planting, have led to increased vulnerability to some crop pests and greater dependency on herbicides; these herbicides, in turn, often increase the susceptibility of some crops to other insect and microbial pests (Oka and Pimentel 1974). This situation stimulates even greater dependency upon insecticides. All of these have contributed to re-establish the viability of traditional agriculture and the necessity of studying folk agriculture in detail.

Apart from the high costs of chemical agents, health hazards are alarming and ominous. Fatal and non-fatal poisoning from pesticides is common, and the long-term effects of ingestion through contaminated food and water are of wide concern (Gorman 1975, 1979; Pimental et al 1977; Taylor 1975).

Another problem is that for cosmetic and psychological reasons many types of insects are considered repulsive (Hosen 1980). That certain species are relished as edible delicacies in one society and viewed with dread or horror in another is a question of cultural "tastes." Yet as Dufour (1981) points out, attitudes toward insects can change. As world food supplies dwindle and long-term space travel becomes a way of life, consumption of insects may have to become acceptable (Pimental 1976). Insects are potentially one of the ideal sources of food and components in waste recycling in outer space because of their light weight, high quality of animal protein, and rapid reproductive rates (Miller 1981). Even so, any major changes in world diet will require a "desensitizing" to produce more positive attitudes towards insects in general and innovative marketing to introduce insect-based products (Dufour 1981).

INSECTS AND MEDICINE

It is in China that we find the most ancient and complete record of the use of insects in medicinal preparations. Read (1935) gives a detailed inventory of useful medicinal species. Chinese veterinary medicine had evolved to a point that curative diets and remedies were used to treat ailing crickets and silkworms (Laufer 1924; Read 1935).

Scarborough (1974b, 1981) gives evidence of the importance of certain insects in ancient Greek and Roman medicine. Numerous other surveys record diverse examples of insects in various cultures (Kevan 1979; Greenlee 1944; Meyer-Rochow 1985; Posey 1978; Swanton 1928). For example, in Brazil termites are used to treat bronchitis, catarrh and influenza, constipation, dog bite, goiter, incontinence, measles, protruding umbilicus, rheumatism, whooping cough, sores, boils, ulcers, etc. The treatments range from teas made from crushed insects or their nests to inhalation of smoke from burning termite cartons (Mill 1982). Principal insect groups listed in Brazil by Lenko and Papavaro (1979) for the variety of their medicinal uses are cockroaches (to treat alcoholism, asthma and bronchitis, colitis, constipation, tooth ache, etc.) and wasps (for stomach ache, wounds, spider bites, constipation, burns, etc.).

Bees are important in Kayapó Indian medicine. Different honeys are though to have different medicinal properties and are used for a variety of diseases. Pollen (collected by bees), larvae and pupae likewise have medicinal qualities. Smokes from different waxes are the most important and powerful curative substances: patients are either "bathed" in the smoke or inhale it. Houses are also "cleansed" by smokes from burnt beeswax, batumen, and resin (Posey 1983b, e, f; Posey and Camargo 1985).

Mixtures of wasps are though to be aphrodisiacs. Parts of the horns of the rhinoceros beetle (*Megasoma acaeon*, Dynastidae) are thought to give great sexual strength (Lenko and Papavaro 1979). Ant and wasp infusions are widely used to cure goiter, paralyses,

and rheumatism (Ealand 1915). Perhaps most amazing is the use of stinging ants and wasps as cures for crippling arthritis (cf. News and Comments, *Journal of Ethnobiology* 3(1):97, 1983). Stings from these Hymenoptera are apparently effective in curing arthritis. Cures for certain types of blindness are also attributed to wasp stings (Araûjo 1961). The Uapixana and Tirió Indians also use ant stings to cure various maladies (Lenko and Papavaro 1979). In Brazil the enormous mandibles of *Atta* are used to suture wounds. The ants are allowed to bite the sides of the wound; when they close their jaws, their heads are broken off and the closed mandibles hold the would together (Gudger 1925).

Other diverse uses include Tindale's (1953) observation of grubs being used in Australia by the aboringines as a "substitute teat" to wean their children. The aborigines also commonly treat stomach ache and colds with a liquid prepared from crushed green-tree ants and larvae (McKeown 1944); crushed cockroaches are used to treat cuts (Rudell 1973). Clousdly-Thompson (1976) provides a long list of medicinal uses of insects in the ancient and modern world.

BEEKEEPING AND INSECT REARING

From ancient cave paintings of honey raids to beekeeping in Babylon, and the use of beeswax to embalm the dead in Assyria, Ransome (1937) traces the importance of "the Sacred Bee" in ancient times. Crane (1984) describes the "archaeology" of bee-keeping recorded in historical texts and art. Crane (1979) also provides a comprehensive survey of production, collection and use of beeswax in many parts of the world. The literature is so extensive on the keeping of *Apis* in ancient and contemporary times that it cannot be reviewed herein.

Keeping of stingless bees (Meliponinae) is a much less known area of ethno-entomology. Yet the keeping of meliponine species was a highly developed science amongst native peoples of Africa and the Americas (Parent et al 1978; Schwarz 1945, 1948).

Schwarz (1948) provides one of the most complete studies on the domestication of meliponines by the Maya of Central America. These Indians were expert in the genetic manipulation of different bees to increase honey and wax productivity and perfected many methods for the division of colonies and the rearing of numerous species. Highly ornamented man-made hives were employed in special shelters constructed for the sacred bees and bee gods. The Maya had several methods to attract and "tame" wild swarms, which included attraction with plantations of flowering plants preferred by the bees. Such practices continued into modern times and are still observed in Mexico, Panama, and other parts of Central America (Bennett 1964, 1965; Hendricks 1941; Weaver and Luhrmann 1981; Weaver 1981).

Stingless beekeeping was also highly developed in pre-Colombian South America. Nordenskiöld (1929) provides an interesting survey of South American folk apiculture observed during the first half of this century.

For stingless bees in Brazil, Lenko and Papavaro (1979) record 171 folk names, the majority of which were of indigenous origin. Many scientific names are actually taken directly from their Tupi Indian language origin (Nogeira-Neto 1970).

Although some species of Meliponinae were undoubtedly fully domesticated in South America, many species were only semi-domesticated. Chagnon (1968) and Metraux (1948b) describe bee management by the Yanomamo and Guarani Indians, but they fall short of describing the bees as fully domesticated.

Contemporary Kayapó Indians of Brazil name and classify at least 56 folk species of stingless bee (Posey 1983e, f); nine species are semi-domesticated. Hives of these species are raided, and a portion of the brood comb (with some honey and pollen) is returned to the next before resealing, so that the bees re-establish the colony and the Indians continue to exploit the hive in subsequent years. In addition to the nine semi-domesticated

species, the Kayapó mark the nests of several others and carefully observe the progress of the colonies. When the Indians think that the quantity of honey and/or wax sufficient, the nest is raided. Nests of some species, when found in the forest, are actually brought to the village to be observed on a daily basis (Posey and Camargo 1985). Other social insects reported to be "managed" include the sauva *(Atta* spp.) (Lenko and Papavero 1979), various wasp species (Baldus 1937; Chagnon 1973; Metraux 1984a), and honey-producing wasps *(Brachygaster)* (Lenko and Papavaro 1979).

Beetle grubs (Phalaenidae and Buprestidae) are raised by several South American tribes (Chagnon 1968; Stewart and Metraux 1948). Palm trees are deliberately cut to provide a fodder for egg-laying adults. The Indians know exactly when to return to the decaying pith to extract the numerous, large grubs. Coimbra (1984) offers detailed information on the rearing of four species of *Bruchidae* and *Curculionidae* larvae by the Suri Indians of Rondonia (Brazil).

A sizeable bibliography exists regarding insect-rearing for laboratory experiments as well as livestock food (Calvart et al 1969; Chambers 1977; MacHargue 1917; Vanderzant 1974). One interesting study deals with the commercial management of *Hermetia illucens* larvae, which are used as fish bait in Brazil (Santos and Coimbra 1984).

Insects have been reared for a number of purposes. Laufer (1927) describes in detail how elaborate carved gourds and miniature houses were prepared for singing and fighting crickets with China (Fig. 1). Special shelters were prepared for them, during the summer, with clay beds built for each individual. Elaborate diets were recognized for different species in different lunar cycles. Special diets and medicines were available for ailing crickets. Intricate porcelain dishes were created for feeding the prized individuals. Even delicately carved "ticklers" were created to urge reluctant cricket warriors to battle. Kevan (1979) notes that American Indians likewise raised crickets "simply to enjoy their songs." Similar reports are found in Bates (1862), Caudel (1916), and Floericke (1922). Posey and Camargo (1985) report on the keeping of stingless bees purely because of the Kayapó Indians' fascination with social insects. Of course, cultivation of silk worms *(Bombyx*

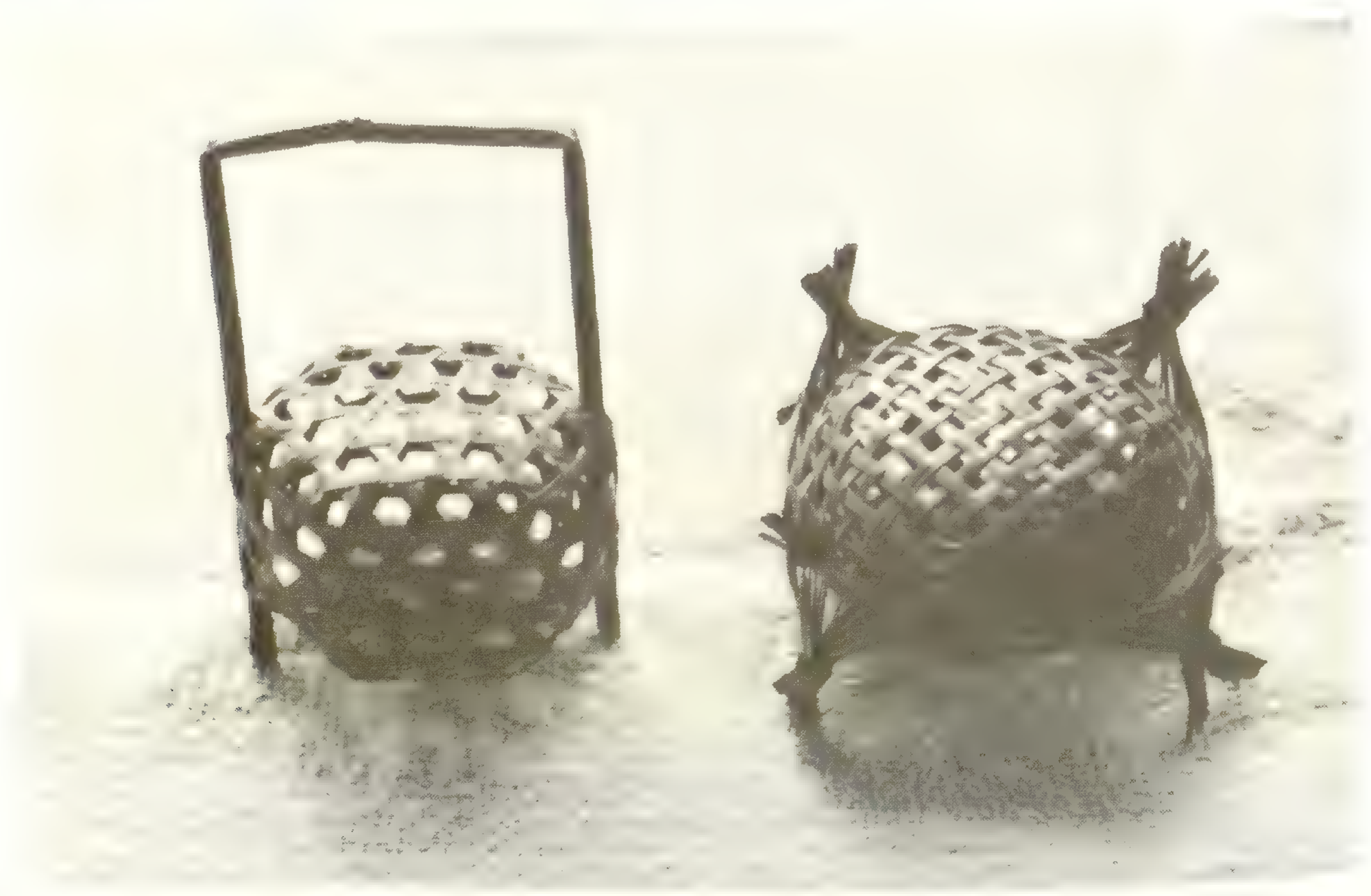

FIG. 1.—Special baskets used for keeping singing crickets in Asia. Courtesy of Dr. Nelson Papavero, Museu de Zoologia, São Paulo, Brazil.

mori) is another ancient Chinese tradition in insect rearing. The details of this folk science are described by Read (1935) and Cloudsly-Thompson (1976).

Scale insects *(Coccus cacti)* that feed on prickly pear *(Opuntia* spp.) are still raised in Mexico, Honduras, the Canary Islands, Algeria, Spain, and Peru because of their use in the production of the carmine-red pigment cochineal (Cloudsly-Thompson 1976; Ealand 1915). Similarly, "lac insects" *(Laccifer lacca)* are reared in Thailand, Burma, and India for their production of shellac, polishes, and sealing wax (Cloudsly-Thompson 1976).

PESTS AND PEST CONTROL

Was it a tarnished plant bug that caused the potato rot which led to the great Irish potato famine? Quite possibly, according to Wheeler (1981). If so, it probably was not the first ecological and social disaster wrought by insect pests. Although the roles of insects in agricultural history remains little known, we do know that today worldwide deforestation and the dominance of crop monocultures have provoked a sharp rise in insect pests (Cooper and Tinsley 1978; Thresh 1982). Likewise there have been dramatic increases in insect-borne diseases caused by blood parasites and arboviruses. These situations, along with the high costs of pesticides and energy for their applications, have stimulated a refreshing new emphasis upon studies of pest management in traditional agriculture (Altieri 1985).

Western agriculturalists have generally assumed traditional agricultural systems to be of low productivity and have used "bigger yield" as their justification for expensive technologies and chemical dependency (Alverson 1984). Yet in many cases, native agriculture has been shown to be both productive and efficient in its use of local skills, available energy, and materials (Egger 1981; Kerr and Posey 1984; Parker et al 1983; Posey 1983c, d; Wilken 1977). One of the major reasons for this effectiveness is efficient pest management.

Traditional cropping systems have "built-in suppression mechanisms" (Altieri 1983a, b). These include: (a) arrangement of crops, (b) composition and abundance of non-crop vegetation in and around fields, (c) genetic diversity of domesticates and semi-domesticates, (d) matching of soil varieties with crop varieties, (e) "natural corridors" between fields, and (f) variation of field sites and long-term management of old fields (Altieri 1985; Denevan 1971; Denevan et al 1984; Parker et al 1984; Posey 1984).

Brown and Marten (1984) point out that crop losses in native fields may be as high as 40%, and such losses are still within the range of losses in modern agriculture using pesticides. One major difference exists: elimination of pesticides from modern systems can produce losses approaching 100% (Schwarz and Klassen 1981), whereas pest damage in traditional agricultural systems almost never exceeds reasonable bounds (Altieri 1985).

The relationship between agricultural polycultures and lower pest incidence is currently under investigation (Altieri and Letourneau 1982; Perrin 1980; Risch et al 1983). Maintenance of a broad genetic base certainly diminishes attacks from host-specific pests (Brush 1982; Gliessman et al 1981; Pimentel and Goodman 1978). A variety of management techniques have been described for different societies. Use of resistant native cultivars, crop rotation, variation in planting times, and use of shade to shelter useful insects are only a few keys to successful traditional agriculture. In Nigeria, for example, okra is planted to divert flea beetles *(Podagria* spp.) from cotton (Perrin 1980). Variations in relative corn and bean planting dates are also used to reduce leafhopper and armyworm damage (Altieri and Letourneau 1982). Many studies detail other management techniques (Altieri 1983a, b, 1985; Bunting 1972; Glass and Thurston 1978; Golob et al 1982; Huis et al 1982; Khan et at 1978; Litsinger et al 1978a, b; Matteson et al 1984; Wilken 1977).

The effects of spatial arrangement, eg. row spacing, are still little known but appear to have significant impact on pest management. Matteson et al (1984) document a signi-

ficant difference in crop loss between cowpeas *(Maruca testulalis)* planted in intra-rows rather than inter-rows with maize. Kerr and Posey (1984) report that interdispersal of *aria (Calathea aloua)* with tuber crops reduces nematodes and *Collembola*-borne virus attacks in Kayapó fields.

Management of "weeds" is also an important factor in the overall practices of traditional agriculturalists. "Relevant weeds," according to Altieri (1983a, b; 1985), support rich natural enemy fauna that provide alternative prey/hosts, pollen or nectar, or favorable microhabitats unavailable in weed-free fields. Most of what Western agriculturalists would consider "weeds" in a Kayapó field are, in fact, useful semi-domesticates for the Indians (Posey 1986). Altieri and Letourneau (1982) provide examples of cropping systems in which the presence of weeds has enhanced biological control of insect pests.

Western science has only begun to seriously study traditional agricultural science. Yet existing evidence already points to the richness of ideas and data available to interested researchers (Brokenshaw et al 1980; Posey et al 1984; Parker et al 1983). Some laboratories have begun the serious study of toxicological potentials of native pesticides. Results have been promising (Ganjian et al 1983; Kubo and Matsumoto 1984; Kubo et al 1984). Other entomologists, agriculturalists, and ethnoentomologists should devote more attention to the investigation of integrated pest management by native peoples.

MYTHOLOGY, RITUAL, AND "NATURAL MODELS"

Reports of insects in mythology and ritual are widespread. Bushnell (1910) and Mooney (1972) discovered many insects as key figures in the belief systems of Indians of southeastern North America. For the Louisiana Chocktaw, for example, grasshoppers and men were created at the same time and were once brothers; ants were likewise considered as having human ancestors (Bushnell 1910). Ant clans existed in many tribes (Gilbert 1943; Grinnel 1899) and ant people were thought to have been the first to have inhabited the underworld (Bushnell 1910). Water beetles *(Hydrophilidae)* were responsible for the formation of the earth because they had brought up the mud from beneath the waters to form the first dry land (Mooney 1972).

The Cherokee attributed the origin of "Sacred Fire" to the heroic efforts of the water spider, which brought fire on its back while corssing the ocean (Mooney 1972). Diseases and crop pests were cast upon people, according to legend, by Grubworm, who organized his fellow insects to punish humans for their abuse of nature (Posey 1977; Swanton 1928).

Insects also play an important role in Australian aboriginal lore (Meyer-Rochow 1985). Spencer and Gillen (1899) reported 30 insect totems; Berndt and Berndt (1984) provide further evidence of insect-named clan and totemic groupings. One of the major cosmogenic myths of the aborigines refers to the famous witchey grub that served as humankind's first food. Numerous examples of insects in mythology can be found in various compendia (eg., Armstrong 1970; Clausen 1954;; Cowan 1865; Denton 1968; Ealand 1915; Griaule 1961; Kevan 1974, 1979, 1980; Posey 1978, 1980, 1981; Reim 1962; Ritschky 1981; Schimitschek 1968, 1977; Wyman 1973).

Insects are also important components in many ceremonies. The Cherokee shamans employed many insect names in their sacred chants (Kilpatrick and Kilpatrick 1970) and had an elaborate "extraction" ritual to remove disease-causing insects from their patients' bodies (Greenlee 1944; Lawson 1937; Morphi 1932). Fortune-telling rituals used insects as indicators of the future (Mooney 1972). In Brazil, one of the most dramatic rituals is that of the Maue marriage ceremony (Biard 1862). Young boys are submitted to an ordeal of pain in which tocandeiro ants *(Paraponera clavata)*, known for their extremely powerful stings, are placed into a woven mitt (Fig. 2). The boys receive dozens of painful stings when they don the ceremonial glove. When the severe swelling subsides from his arm, a body is considered free to marry.

FIG. 2.—Ceremonial mitt used by the Maué Indians of amazonia. Stinging ants are placed in the mitt, which is then worn by boys in the marriage ceremony. Courtesy of the Museu Paulista, São Paulo, Brazil.

Accounts such as those previously related are generally recorded out of cultural context and, consequently, are of limited significance to the folk entomologist. Recent studies (eg., Brown and Chase 1981; Gregor 1983; Luhrmann 1981; Malkin 1956; Posey 1985; Waddy 1982; Wilbert 1981) have attempted to provide a broader cultural framework for the interpretation of insects in myth and ceremony. Natural "models" based upon insect examples and recognized by native peoples themselves have been shown to be useful for the organization of folk scientific data and are significant alternatives to the imposed models of traditional anthropological structuralism and Western science (Posey 1981).

ORAL LITERATURE AND ECOLOGICAL KNOWLEDGE

Oral literature is a major vehicle for ecological information. Santos and Posey (1986) witnessed an old man in the Ilha de Lencois (Brazil) describing his pursuit of a mythological animal. The "plot" of the story could take only three minutes to relate, but the master story-teller kept his audience of local youth spellbound for nearly 45 minutes. Analysis of the folk tale reveals that the minute details used to embellish and add credence to the story is also instruction in local ecology and survival.

Myths are concentrated symbolic codes that transmit cultural information, including social rules and standards of behavior. Ecological information, such as knowledge of animal behavior and "coevolutionary complexes," can also be communicated in myth form (Posey 1983c). Baldus (1937, 1970) recorded Taulipang myths describing the commensal relationships between birds and wasps. Kayapó lore describes commensalsim between stingless bees and acrids (Posey and Camargo 1985).

Mill (1982) points out that widespread stories of "weeping termites" in Brazil reflect folk knowledge of the biological fact that ground nesting termites *(Nasutitermes, Velocitermes,* and *Cortaritermes)* exude droplets of exocrine secretions for chemical defense when disturbed. A Kamaiura myth describes termite nests that glow in the night (Villas-Boas and Villas-Boas 1972). These glowing nests are not superstitious nonsense, but rather recognitions of a natural phenomenon caused by periodic invasions of termite mounds by phosphorescent *Lampyridae* larvae (Redford 1982).

Oral literature has not been sufficiently studied as a transmitter of biological information. This is because of the highly symbolic language of myth and folklore, which is frequently considered as nonsense by those who do not understand the linguistic and cultural codes. Researchers who take the time to learn the language of the societies they

study and prepare themselves with training in folkloristics can indeed make a significant contribution to myth interpretation and ethnoentomology.

MISCELLANEOUS TOPICS

Several miscellaneous topics deserve mention. Brief examples will be given to illustrate each topic.

Use of insects as ornaments and decorations—Berlin and Prance (1978), Covarrubias (1971), Kennedy (1943), Lothrap (1964), and Outram (1973) review the importance of insects in art and ornamentation in the New World. Meyer-Rochow (1975) reports the use of green tenebrionid beetles, as well as scarabaeids and buprestids, among the Wahgi Valley people of New Guinea. The Kayapó inherit the right to use irridescent elytra of *Euchroma goliatha* and make elaborate ceremonial hats from meliponine batumen (Posey 1983e). Butterfly wings are commonly used in the Americas for adornment and decoration (Posey 1986). Klots and Klots (1959) report the use of luminous beetles *(Pyrophorus* spp.) to decorate the hair of Indian girls.

Insects as objects of entertainment—Dragonfly catching is a favorite and developed sport in the Banda Islands (Simmons 1976). Butterfly wings are important play objects for Trobriand Island youth (Meyer-Rochow 1985). In Papua New Guinea, large weevils *(Rhynchophorus ferruginius)* are used as musical instruments by letting the human mouth serve as a variable resonance chamber for the wing vibrations of the beetle (Meyer-Rochow 1973). Staged fights between lucanid beetles are reported in Thailand (Meyer-Rochow 1975). Posey and Camargo (1985) report the keeping of stingless bees by the Kayapó Indians purely because of their fascination with social insects. Lenko and Papavaro (1976) give several examples of the keeping of *Pyrophorus* spp. beetles for entertainment, as well as for their light. Dances inspired by insect movements are reported in several North American Indian groups (Bushnell 1910; Gilbert 1943; Schoolcraft 1851; Swanton 1928, 1946).

Insects as indicators—Due to the sensitivity of the head louse to minute changes in body temperatures, some native groups diagnose illness of patients by the presence or absence of lice. Slight fevers can cause an exodus of body lice that indicates oncoming illness (Malinowski 1929; Raths and Biewald 1974). Absence of certain insects can be taken as a sign of environmental pollution (Englehardt 1959), while the presence of other species (such as green flies that are attracted to decaying matter) can indicate unhealthy conditions (Meyer-rochow 1985). Water striders, for example, are indicators of polluted water to the Trobrianders (Meyer-Rochow 1985). Meyer-Rochow (1985) reports how Australian aborigines use the contents of spider webs to indicate the proximity of honey bees. The presence of "mutucas" *(Tabanus)* near river banks indicates the presence of game to indigenous hunters of Brazil (Lenko and Papavaro 1976).

Insects as UFOs—Many tribes in Papua New Guinea report the presence of "flying light spots" in areas where luminescent creatures are not reported by entomologists (Callahan and Mankin 1978). These sightings may be explained by flying insects that enter into electric fields caused by thunderheads; the result is that ordinary insects appear to "emit sparks" (Meyers-Rochow 1985).

Insects and utilitarian concepts—Insects are frequently used as fish bait in preliterate cultures (Kevan 1979). *Nasutitermes* mounds are used for construction material by Brazilian Indians, who prize the natural insulating qualities of the nests extensive galleries (Posey 1979). Nests of *Azteca* ants are buried with newly planted crops to stimulate plant growth (Kerr and Posey 1984). Beeswax and batumen are extensively used for artifact production and paint bases by indigenous tribes of South America (Crane 1979, 1984;

Posey 1978, 1980; Schwarz 1945, 1948). In Papua New Guinea and Northern Australia, ants and maggots are used to clean skeletons and bones. Cantharid beetles are the source of poisons for arrow points for some South American Indians (Meyer-Rochow 1985).

Hallucinogens—Insects have been found as sources of hallucinogens used by some indigenous groups (Meyer-Rochow 1985). It is unclear if the hallucinogenic properties are due to the insects themselves or the plant sources upon which they feed (Blackburn 1976).

Insects and Archaeology—Insects are frequently found in archaeological sites. The presence of seasonal species has been shown to be useful to the archaeologist in determining seasonality of site use and the historic ecological setting (Gilbert and Bass 1967; Hevly 1982; Hevly and Johnson 1974).

Urban Ethnoentomology—A current topic in entomology is the ecology of insects in urban environments (Frankie and Ehler 1978). Studies in this area focus upon the adaptations of insects to the special climatic and edaphic conditions created by intense human manipulation of the natural environment. "Synanthropy" describes the nature of this coexistence with humankind over an extended time (Povolny 1971); a formula for determining the degree of synanthropy has even been developed (Nuorteva 1963). This specialized area of human-insect relationships might also be called "urban ethnoentomology."

HYPOTHESIS GENERATION: THE ETHNOBIOLOGICAL BRIDGE

The interdisciplinary Kayapó project has developed methodological procedures to scientifically test hypotheses generated through its ethnobiological investigations of indigenous ecological knowledge (Posey 1986). Native concepts and beliefs are used by Western scientists as *emic* guides for their research designs (Posey 1983a, 1985). Data collection utilizes indigenous categories for floral and faunal inventories, while ethnoecological concepts (often couched in myth and natural symbols) establish the basis for interdisciplinary dialogue and research. In this manner, indigenous knowledge of biological communities and ecological relationships can be studied; when non-Western notions arise, these are formulated as hypotheses and tested by respective specialists.

Posey (1983b), for example, reports the discovery of nine new species of stingless bees (Meliponinae) through the comparison of Kayapó and Western taxonomic systems. Posey and Camargo (1985) record the utility of Indian knowledge about bee behavior in the development of studies in areas little known to ethnoentomologists, such as: differences in odor characteristics, swarming behavior, flight patterns, and habitat choices between or within meliponine species. They also propose scientific investigations based on indigenous knowledge of bee species distribution in relation to ecological zones and habitat sharing by certain species clusters. Indian ideas of acrid commensalism and use of odor trails by species for which such activity is unreported have also spurred further studies by entomologists of stingless bee behavior.

Overal and Posey (1986) have effected a large inventory of arthropod agricultural pests based on Indian information and confirmed by field collections. They also report the development of research into the highly effective control of agricultural pests in indigenous gardens through inter-cropping, use of trap crops and natural predators. The Indians attribute much of this natural control to predatory ants, wasps, and termites, all of which are glorified in Kayapó myth and song. Roles of these insects in crop pest control are being investigated following indigenous guidelines.

Kerr and Posey (1984) report how the Kayapó utilize *Azteca* spp. ants to repel leaf-cutting sauva (*Atta* spp.). Likewise, Kerr and Posey (1986) report the indigenous use of

several natural pesticides and call for their testing by Western science. At least in the case of *Azteca* spp., Overal and Posey (1986) report very positive results from scientific tests to determine their effectiveness in the protection of Amazonian citrus.

Anderson and Posey (1986) and Posey (1984) report the intentional planting of certain plant species by Indians to attract bees. Such knowledge can be helpful in the investigation of tropical pollination and aid in the improvement of apiculture.

Many bee species are thought by the Kayapó to have important medicinal properties (Posey 1983e). Although almost practically unknown by pharmacologists they need to be investigated for their effectiveness and potential for a natural pharmacopeia (Elisabetsky and Posey 1986).

These are but a few examples from a single ethnobiological project of how indigenous knowledge can stimulate new ideas for Western science. No researcher is expected to accept *prima facie* all native beliefs. Much indigenous knowledge, as we have already seen, is highly symbolic and difficult for even the most experienced anthropologist to interpret; however, nothing can be dismissed by the ethnobiologist no matter how ridiculous it may intially sound. The most seemingly ludicrous ideas today may offer the greatest insights tomorrow when their symbols are finally decoded.

Refusal by Western scientists to study native beliefs is, after all, not a very scientific attitude. It is much more scientific to test the validity of native observations through the testing of hypotheses generated by ethnobiological study.

CONCLUDING REMARKS

Knowledge, classification, and use of insects in human societies is diverse but relatively unstudied in a systematic manner. Lack of anthropological and linguistic training by entomologists—and entomological training by anthropologists and linguists—hampers ethnoentomological research. A true science of ethnoentomology will not develop until researchers have sufficient expertise in all three fields to investigate the native *emic* view of "natural worlds."

This situation does not prevent the elaboration of studies in cultural entomology that attempt to investigate insect importance in literate societies. Nor does it inhibit important research into the potential uses of insects as foods and medicines. Indeed, insects have played a significant role in human history and may be even more important in the future. Whether as protein sources in space flight or as key elements in integrated biological pest control, insects will continue to be studied and manipulated for human welfare.

From the theoretical side, folk biological studies can discover "natural models" used by other peoples to define their own world in their own terms. Instead of imposing paradigms of anthropological structuralism and Western science upon non-Western peoples, we must learn to elicit and organize our data within the cognitive bounds of the societies we study.

Folk systems of knowledge have in most cases developed for many millenia and are frequently more ancient than Western science. They reflect the diversity of ways in which the natural world can be ordered and provide detailed information of ethology, ecological communities, useful species, and biological diversity. Folk knowledge can also serve to generate new ideas and hypotheses that can be investigated and tested with the rigorous controls of occidental science.

Studies of folk knowledge as outlined in this paper offer a powerful "intellectual bridge" between different peoples. Understanding the sciences of other cultures enriches Western science and provides the philosophical bases for the understanding and appreciation of other peoples *on* and *in* their own terms.

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NEW DIRECTIONS IN ETHNOPHARMACOLOGY

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ABSTRACT.—New directions in ethnopharmacology must be concerned with more than the recording and preservation of folk knowledge of plant and animals species with biodynamic compounds. Native concepts of disease and illness, collection and preparation procedures, religious and ceremonial contexts, etc. must also be studied and understood. Such additional information will aid in identifying prototype drugs as well as their development and return to the indigenous cultures from which they came.

The growth of Science would depend on man's willingness to believe the improbable, to cross the dictates of common sense.

Daniel J. Boorstin

INTRODUCTION

Several workers (Farnsworth and Morris, 1976; Farnsworth and Pezzuto, 1983; Holmestedt and Bruhn, 1982) in discussing the historical importance and potential of ethnopharmacological research have emphasized that such studies may lead to (1) the discovery of prototype drugs[1]; (2) the recognition of new therapeutic actions of compounds already commonly in use for other purposes (Peigen 1983); (3) the discovery of new sources of crude material for semi-synthetic drugs; and, (4) the utilization of *in natura* plants or their simple pharmaceutical formulations as a means of attaining the development of low cost medicines. Some 7,000 compounds currently used in modern medicine are derived from natural products; most had been used for centuries by European, Asiatic and Amerindian healers. This can be better appreciated when one considers that even in highly industrialized countries, 40-45% of the commercial pharmaceutical products come from natural sources. For the USA in 1980, this amounted to US$ 8 billion (Jaroszewski 1984; Farnsworth 1985). The contribution of natural products to world health care is even greater in Asia, Africa and Latin America, where a large portion of the world's population lives. There, either by choice or necessity, plants are used as medicine directly from nature (Svendsen and Scheffer 1982).

Various persons are often repositories of a great deal of knowledge of natural medicinal sources within isolated indigenous and peasant cultures. There is now a considerable danger of loss in the oral transmission of medical plant lore to the youth of the societies because of pressure from economic development and accelerated interaction with dominant cultural systems. Unless this trend changes or unless the information is recorded, this valuable knowledge will be lost. Reversing this trend will require detailed ethnographic research which is crucial for the understanding of drug use and medical practices of a given society, because these are inextricably integrated with religion, ritual, social relations, etc. Native knowledge and understanding of medical concepts is

fundamental for ethnopharmacological research, for only then can we understand why and how such a treatment is used.

Interaction between anthropology and ethnopharmacology is the basis upon which holistic research on medicinal plants in particular, and medical care in general, must emanate (Elisabetsky & Posey 1985; Elisabetsky & Setzer 1986). The purpose of this paper is to elaborate the linkages between anthropology and ethnopharmacology, stressing their relation with further laboratory procedures.

ETHNOLOGY AND DRUG EFFECTIVENESS

The concepts of health and disease, as well as the medical system as a whole, are articulated within a given society, as cultural systems (Kleinman 1978). Although anthropologists frequently stress that indigenous societies deal with "sickness" as interrelation between nature, supernature, society and the individual (Fabrega 1975; Tsunoo 1978; Worsley 1982; Young 1982 and others), ethnopharmacologists nevertheless, often select traditional medicine to be screened by laboratory procedures in hope of the discovery of new universally applicable drugs without considering their cultural contexts. This leads to a shortcoming because not every traditional remedy is necessarily acting upon the physiological processes responsible for the proper functioning of the organism. Since a truly holistic view of the individual person is the reference basis for indigenous medical reasoning, treatments are often directed to possible sources of sickness other than physical ones. Thus, religious/ceremonial aspects integrated into indigenous medical systems are viewed as a way to cope with psychological needs, social control, and adoptions of preventive steps (Fabrega 1975) instead of just "folk" explanations or "superstitions".

Although the necessity of interdisciplinary work is frequently stressed (e.g., Bruhn and Holmstedt 1981), most ethnopharmacologists consider anthropological data merely as a catalogue of remedies, methods of preparation or simple lists of medicinal plants. Even this very fundamental information is often not considered in the overall research designs. It is because medicinal plants (or animals) are used in different therapeutical contexts that ethnopharmacology requires an interdisciplinary approach that includes anthropologists.

Ethnological studies deal with disease concepts, belief systems and attitudes as conceptual frames of references, stressing their internal logic and coherence (Quintanilla 1978; Young 1982; Worsley 1982). The position of many anthropologists may thus be limiting to ethnopharmacology for they preclude the possibility of attaining universal medical advances based on different cultural experiences. We cannot disregard the possibilities of attaining new medical ideas by integrating traditional and modern knowledge. For instance, this comprehensive view of the individual, so common and fundamental in indigenous societies in its approach to health or sickness, is now only receiving much attention in our own medical science, thanks to recent development of such new disciplines as psychosomatic medicine, medical ecology and the recognition of the true importance of social conditions in relation to individual and public health. It is well to note that, in relation to prototype drugs, medical practices would likely change for "more frequently than not, it is the availability of drugs that permits new biochemical, physiological and specially, pharmacological knowledge to accrue. This in turn often allows for the theoretical rationalization of human disorders" (Maxwell 1984:377).

The success of this interdisciplinary approach lies, therefore, in the understanding of the physical and cultural treatments of a holistic medical system. Only then can we isolate and scrutinize particular aspects of medicine (Elisabetsky and Setzer 1986).

GOALS OF ETHNOPHARMACOLOGY

Although to many "the goal of ethnopharmacologic research is to find new prototype drugs" (Malone 1983:134), this is not and should not be considered the only one. If, for example, a country dependent on foreign sources of a certain pharmacological compound (see Gereffi 1983) detects, through ethnopharmacologic research, another source of this compound in a native species, a critical first step will have been taken in making that drug available for lower costs. For developing countries, where importation of pharmaceuticals is is one of the fastest growing drains on hard foreign currency (Antezana 1981:175), this extremely valuable contribution from ethnopharmacological research, i.e., a national source of crude material, is necessary before the drug can be developed and promoted (see WHO 1979). To take specific examples, in China there are 200,000 people engaged with the planting, processing and distribution of medicinal herbs, including 800 pharmaceutical industries producing 2,000 types of remedies (Wang 1983). In India, there is a network of more than 600 hospitals and 15,000 dispensaries providing health care facilities based on traditional medical systems such as Ayuveda, Unari and Sidha (Akerele 1985).

From those plants, today used as crude materials, many will eventually be transformed in the so-called prototype drugs (Peigen 1983). Nevertheless, a great part of China and India's populations are already profiting from their herbal lore, long before Western science achieves its goal. Different goals may require procedures other than those now used by pharmaceutical industries and/or some academic laboratories where the complete pharmacological evaluation of each plant coupled with the complete phytochemical analysis of the whole plant (or the part used by natives) is undertaken (see Malone 1977, 1980). This is an unrealistic goal for most laboratories in developing countries because of high cost and unavailable technology. The ultimate goal of complete evaluation and analysis is the patent of a new molecule and this may not be the only—or even the most important— goal to Third World countries. Although complete laboratories may be rare in developing countries, some specialized in specific pharmacological/therapeutical actions can be found. Indigenous knowledge, and its evaluation through ethnological research, coupled with biomedical data, becomes crucial since therapeutic action indicated by users can give the clues to those physiological processes most likely related to the action of the active compound(s) of the indigenous medicine (Peigen 1983).

The possibility of correlating the native therapeutical use with our own biomedical concepts would therefore fit important roles for ethnopharmacological research in developing countries: first, it permits the selection of the correct disease experimental model, that is, the one that would most likely reveal the pharmacological activity necessary to attain the described therapeutical action; secondly, it would allow researchers to direct plants to the appropriate laboratory for analysis and testing. Once this laboratory is provided with the means to make the complete analysis regarding one specific pharmacological action, it would be able to analyse many other plants used for the same purpose with medium costs. Reduction of costs at the research/development stage would be reflected in the cost of the final product (Elisabetsky 1985).

INDIGENOUS PHARMACOTECHNICS

Bernardi (1980) reminds us " . . . we (scientists) cannot but consider medicines as natural objects, but we have to visualize them in their cultural background as the products of man, aimed at being used for the health of man" (Bernardi 1980:95). As Evans-Pritchard (1937:448) put it: "medicines are not natural objects but human artifacts." Native users, though, are well acquainted with the importance of different steps to be

followed in order to transform a plant into a medicine. As different parts of the same plant may vary according to their chemical constituents (qualitative or quantitatively), the part selected by natives is of critical importance for phytochemists as well as pharmacologists. It is known that environmental factors may alter chemical constituents (Brown 1984). Users, for example, may state that a certain plant is more effective when collected along the river than this very same species collected in any other place. Or, they may note that a plant is suitable for making the remedy only if collected in the summer, because it is toxic when gathered in winter. Since we are increasingly becoming aware of the importance of such environmental factors as sources of modification of chemical profiles of plants, such information should be recorded and carefully analysed.

There are several traditional ways to prepare remedies, e.g., concoctions, infusions, cold or hot teas, baths, alcoholic infusions, fermented beverages, heated leaves for plastering, roasted seeds for coffee-like beverages, cigars made out of dried flowers or leaves, enemas, extracted oils and saps, syrups and still many others made by natives (Elisabetsky and Setzer 1986; Posey and Santos 1986). The quality and/or quantity of the substances that will be extracted varies significantly with each of these procedures. Methods of preparation may indicate the means by which an indigenous population determines the best therapeutical use of a particular species (i.e., by extracting the active substance, eliminating the toxic ones and, determining the appropriate mode of administration). Indigenous methods may, and most likely will, be improved through the use of modern technology, but should be taken as important clues in beginning stages of research.

It is a common practice among natives to mix more than one species of plant in making a medicine. This can have two basic consequences to further laboratory analytical procedures: (1) it may be possible that only one of the several plants used is actually the one responsible for the desired pharmacological action, although this plant may not be reported as the most important in the remedy; (2) since drugs interact with each other, the presence of one may alter the pharmacological properties of another. There is also the possibility that all or several of the plants used in the mixture have the same pharmacological action that adds to overall effectiveness.

Posology (the administration mode and dosage regime) determines the effectiveness of any kind of medicine. Natives can give specific information regarding the posology of their medicines. The same plant, prepared and/or ingested in different ways, is often used for different purposes. Even baths and massages, widespread practices in several indigenous medical systems, as well as among caboclos, are now being reevaluated since new evidence regarding skin permeability may offer a scientific basis for their claimed efficacy (Lewis and Elvin-Lewis 1977). I suggest that, if all these items are carefully taken into account, the results of ethnopharmacological studies can be improved.

INTERACTION BETWEEN MEDICAL SYSTEMS

Contacts between Western medical care and indigenous concepts of disease, diagnosis, and cure are continuosly growing throughout the world. Contradictions in conceptual frameworks are apparent and it is clear from ethnographic studies "how ineffective modern therapeutic programs can be when medical personnel fail to gain a thorough understanding of the principles and concepts of traditional medicine that govern the behavior of these patients" (Logan 1978; Rubel 1967). Chen (1978:273) properly concludes: "If the dominant system recognizes that it is unable to provide adequately for the care of the population and that other medical systems are filling in this gap for a sizeable part of the population, it has little choice but to accommodate this fact."

There is not necessarily conflict between indigenous curers and their biomedical counterparts. This can be seen in the integration of traditional and Western trained practitioners in China, India, Brazil or Malasya where folk and Western care are both institutionally provided (Elisabetsky and Setzer 1986). It is both possible and necessary to improve the interaction between folk and modern medical systems. It should be possible to return ethnopharmacological knowledge, improved through scientific analysis, to the people that most contributed to it and most desperately need it. We can use this knowledge for their benefit through the distribution of scientifically tested active medicinal plants, advising against the use of toxic ones, providing dried plants (cultivated under controlled conditions in order to optimize and maintain a constant chemical constitution) making them available through the whole year and/or to places where they do not originally exist; and providing simple pharmaceutical (Gallenic) preparations.

Contacts between native and Western medical systems are inevitable. If correctly done, anthropologists can propose health programs that teach the importance of an understanding of and appreciation for local medical theories to western biomedical practitioners (Alvarado 1978; Logan and Hunt 1978; Press 1978; Rubel 1967; Spector 1979; Scrimshaw and Burleigh 1978; Worsley 1982). In addition, programs can be devised for indigenous populations with the purpose of informing them about the deleterious effects of allopathic drugs that are more likely to occur due to their misuse (Illich 1975). Such effects are more widespread in the Third and Fourth Worlds as the result of, among other causes, failures in the control of sales, delivery, distribution, and consumption of drugs. This problem is further complicated by the misinformation that accompany remedies sold in these areas (Silverman et al. 1982). Needless to say, ideological influences and economic pressures that result from multinational pressures, colonialism, and imperialism make the resolution of these problems even more difficult (see Silverman et al. 1982; Van der Geest 1981).

CONCLUSIONS

The discovery of prototype drugs (Malone 1977) is just one of the ways ethnopharmacology can contribute to the improvement of health systems throughout the world. Although a desirable goal, another important contribution is the development of low cost medicines. Figure 1 shows a proposed research model for the various possibilities deriving from ethnopharmacological studies and how these goals can be achieved through interdisciplinary collaboration. It stresses the cruicial role of ethnographic research into the practical aspects of indigenous health care as the first step of such a model. It specifies the sequence of steps needed for collection and classification of specimens involved in plant selection, together with the different levels of treatment that can be worked out from the data.

At this point it should be emphasized that medicinal plants can be used in different therapeutical contexts. This is the area in which ethnobotanists, botanists, chemists, pharmacologists and anthropologists should work together (Bruhn and Holmstedt 1981). The outcome of such interdisciplinary research perspective will be (1) the preservation of knowledge that would be otherwise lost because of the abrupt urbanization and development process occuring throughout the world (Prance 1972; Schultes 1984); (2) the rationalization of ethnopharmacological and phytochemical laboratory procedures with a consequent reduction of investments and a greater possibility to develop low cost medicines (Elisabetsky 1985); (3) the promotion of understanding of the impact and possible damage that a careless and negligent use of allopathic medical care may have upon native populations that comprise the bulk of the planet's population (Van der Geest 1981, 1983).

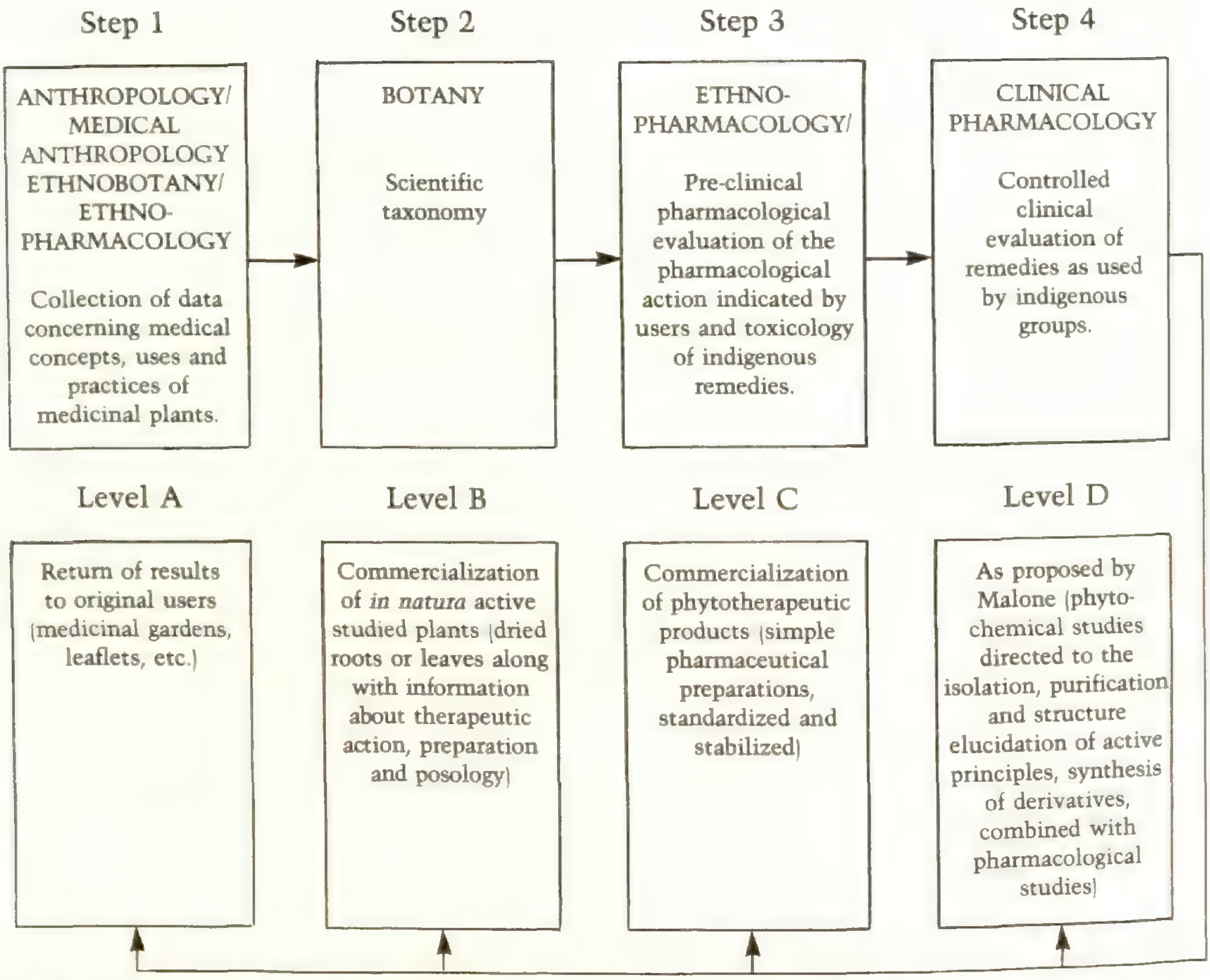

FIG. 1.—Propose line of ethnopharmacological research. Based on Carlini (1983).

NOTE

[1]"one that has a wholly different chemical structure from existing agents and a wholly different medical application. Every time such a drug is discovered, there will be major changes in the practices of medicine." (Malone 1983:128).

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AN INTRODUCTION TO ETHNOVETERINARY RESEARCH AND DEVELOPMENT

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ABSTRACT.—One of the newest directions in ethnobiology, ethnoveterinary research and development (ERD) is no more than a decade old. As this label suggests, ERD constitutes the systematic investigation and application of folk veterinary knowledge, theory, and practice. Common topics in the field include: veterinary ethnosemantics and ethnotaxonomy; ethnoveterinary pharmacology, manipulative techniques, and magico-religious operations; appropriate methods of veterinary extension; and folk management of animal health in the context of the livestock production system as a whole, and its relation to larger ecological, socio-organizational, economic, ideological, and political structures. As "veterinary anthropology," this latter approach characterizes the core of both present and future ERD. Largely stimulated by international livestock development concerns, anthropologists and veterinarians have joined forces to tackle the real-world complexities of ethnoveterinary systems from a holistic but comparative and production-systems-specfic perspective which gives equal attention to emic and etic analyses of animal health-care problems and their solutions. With the integrated knowledge this inter-disciplinary endeavor yeilds, developers can more readily design and implement socioculturally acceptable and ecologically and economically sound interventions to improve animal health and productivity—and with it, the well-being of human groups whose livelihood depends in whole or in part upon animal husbandry.

INTRODUCTION

Ethnoveterinary research and development (hereinafter ERD) constitutes such a "new direction" in ethnobiology that as yet there is not even consensus on a label for the field. "Ethnozootechnics" has been suggested as one possibility (Schillhorn van Veen, pers. com.). Sollod and Knight (1983) and Sollod *et al.* (1984) have coined the epithet "veterinary anthropology." And here I opt for the more generic rubric that forms the title of this review. If labeling this domain of study is somewhat problematic[1] defining it is even more so. Its boundaries are diffuse, shading off at the edges into a variety of different disciplines and subdisciplines in both the hard and the "soft" sciences, and in both "pure" and applied research. If ERD cannot be easily bounded disciplinarily, neither can it be expediently defined—as sometimes done for other "fuzzy" fields—as "whatever an 'ethnoveterinarian' does." No such creature exists!

However, as Sollod *et al.*'s (1984) label indicates, the principal actors in ERD are veterinarians and anthropologists, working both singly and jointly. The latter are almost exclusively sociocultural anthropologists, although occasionally a folklorist, linguist or even an archaeologist may investigate a topic directly or tangentially related to animal health. Among veterinarians, a number of fields are represented: epidemiology, immunology, parasitology, pathology, pharmacology (or pharmacognasy) and physiology. There is also room in ERD for contributions from: many of the biological sciences, e.g. botany, ecology, ethology, entomology, zoology; certainly from specialists in animal

The ethnoveterinary researcher on the job. Above, riding the range: interviewing alpaca herders about veterinary techniques and animal management practices, while also observing forage and water conditions and examining general herd health. Below, the author (unseen) photographs Andean stockowners as they discuss the relative efficacy of folk versus "modern" remedies for the many ills afflicting their sheep.

husbandry, range science and water management; and at the level of veterinary policy, planning, and extension, from rural sociologists, economists, agricultural economists, communications experts and others.

Given this range—both actual and potential—of researchers and their research orientations, a strict definition of ERD is difficult and perhaps not even desirable. However, a very broad definition can be offered: ERD constitutes *systematic* research and development which takes as its principal subject or its major departure point *folk* knowledge and beliefs (theories, taxonomies, definitions, diagnoses, etc.), practices, technology and resources, social organization and so forth pertaining to any aspect(s) of *animal health* among species raised or managed by human beings.

In this definition I have opted for the term "folk" (or in Francophone writings, "populaire") rather than, e.g., "traditional" or "indigenous" merely in the interest of historical precision. The latter two terms frequently appear in ERD titles, but a people's veterinary beliefs and practices are not always entirely or demonstrably traditional or indigenous. Instead, they may represent a melange which incorporates elements from other ethnic groups and/or from modern veterinary science. In the latter regard, folk systems may have absorbed these elements (albeit often imperfectly) through word-of-mouth diffusion, by contact with commercial livestock operations, or from veterinary extension services. In fact, as extension efforts intensify, folk veterinary medicine around the world tends to become ever more syncretic.

Leaving aside this minor terminological point, as for "aspect(s) of animal health," these naturally incorporate all features of livestock production systems which can impact—whether positively or negatively, directly or indirectly—upon the physical condition of the animals being managed. At the broadest level, this includes all husbandry techniques involving: feeding, watering, range and pasture management; manipulation of breeding, reproduction and herd composition and dynamics; housing and supervision; prevention, control, curing of disease and, relatedly, sanitation in all management operations; and harvesting of animal products. From an emic perspective, supernatural husbandry techniques—like reproductive, protective, or propitiatory rites and magical cures for animals—must also be included in this list. Ultimately, too, the larger ecological, economic, political, sociostructural and ideological contexts of the animal production system itself are implied in ERD in its fullest formulation—at which point it in truth becomes "veterinary anthropology."

Having dealt at least provisionally with labeling and defining ERD, the next step is to identify the corpus of work falling within its purview. Here, the definitional qualifiers "principal," "major," and especially "systematic" come into play. Desultory references to folk veterinary beliefs and practices or related husbandry techniques can be found scattered throughout many works. These include: ethnographies[2] of peoples whose livelihood depends upon animals; accounts by travelers, missionaries, former colonial authorities or other officers (e.g., de St. Croix 1972); writings in medical anthropology;[3] archaeological treatises;[4] field-based studies in veterinary medicine and range management;[5] and still others.[6]

Naturally, all such sources of information should be consulted by the ERD researcher in preparation for work among a given ethnic group or on a specific animal health issue. However, they do not fit any definition of ERD *per se*. Either their treatment of matters ethnoveterinary is asystematic, anecdotal, and very much subordinate to a different principal topic (the most common case); or their data base falls wide of the "folk" mark. Just the opposite is true of the works reviewed here. As a first effort at drawing together ERD worldwide, the following introductory review is perforce non-comprehensive.[7] Nevertheless, the studies referenced and discussed below do constitute the bulk of the literature to date, and they accurately represent the variety of thrusts in the field.

DISCUSSION

ERD background, development, and goals.—With one qualification, studies which take folk veterinary beliefs and practices as a primary topic of scientific investigation first began to appear in the mid-1970s. In veterinary medicine this statement is qualified by the longstanding study and use in veterinary pharmacology and pharmacotherapy of herbal remedies for animals (e.g. Bairacli-Levi 1984; Schillhorn van Veen, pers. com.). In anthropology, however, it seems to be unqualified—despite an established interest in the study of domesticated animals from a number of perspectives (Shanklin 1985b). Between the mid-70's and now, ERD can indeed be said to have burgeoned. Predicatably, it is difficult to arrive at many generalizations about the field overall. Researchers come from a variety of countries and disciplines; their research issues, emic/etic emphases, and theoretical approaches (where these exist) vary accordingly; their geographic areas of investigation girdle the globe; the species involved can include any animal domesticate or semi-domesticate; and, of course, the field itself is still in a phase of rapid growth and change.

Where this decade of diversity acquires coherent focus, definition and purpose, however, is in the arena of international livestock development and extension. Here, ethnoveterinary research has as its explicit, overarching goal the enhancement of livestock productivity through improved management of animal health, as informed by an understanding of folk veterinary medicine and related husbandry techniques. Largely with the impetus from development projects like the Small Ruminant Collaborative Research Support Program (SR-CRSP) and the Niger Range and Livestock Project, as of the 1980's a handful of "core" works and workers in ERD have emerged.

This core of ethnoveterinary endeavor is characterized by its holistic, systems-analysis, and therefore interdisciplinary orientation. That is, it recognizes the importance and interconnectedness of the physical, cultural, social, economic, political and historical matrices in which animals and their owners are embedded. It therefore seeks to integrate findings from correspondingly appropriate but disparate disciplines in the biological and social sciences (after Sollod *et al.* 1984:285-286). Additionally but not distinctively, core ERD emphasizes the need for firsthand field research among stockowners themselves, under real-world husbandry conditions, in order to arrive at any meaningful comprehension of this systemic complexity on the ground. To this end, it draws heavily upon anthropological method and theory, combining these with the technical skills and knowledge of animal scientists. It is, in fact, "veterinary anthropology."

This core thrust in ERD has come to the fore only in the last five years, and it clearly charts the course of the field's future growth. As noted earlier, to date it has almost exclusively involved veterinarians and sociocultural anthropologists. And mainly due to present policy priorities in international development, it has so far concentrated upon herd animals (cattle, sheep, goats, alpaca, llama) in Africa and, to a lesser extent, Latin America.

In contrast, the first half of the field's formation displays a greater diversity in researchers, species and geographic locales, although many of the research topics are the same. These "diverse" studies continue to increase[8] in quantity and quality, and much of the data they produce are immediately relevant to core ERD concerns. But again, they are differentiated by their more delimited and disciplinary-specific research goals and approaches. In this respect, the holistic, systemic and ultimately practical thrust of core ERD has lent fresh meaning to the congeries of studies in the field as a whole, placing them into a more unified heuristic framework.

The following discussion is organized by general topical areas which have been addressed in any part of ERD to date. Throughout, the relevance of each area to develop-

ment and extension is highlighted. The topical categories themselves are not discrete; they merely serve as an organizational device. Many studies in fact span a variety of categories. Due to their holistic orientation, this is particularly true of core works. In such cases, studies are often cited and/or discussed in several sections.

Veterinary ethnosemantics[9] *and ethnotaxonomy*. I begin with this area because it forms the backbone of almost any endeavor in ERD. The importance of even the most basic semantic and taxonomic researches for determining and analyzing indigenous veterinary and husbandry concepts and how these guide behavior, for identifying different types of native veterinary practitioners, and for communicating with stockowners and extending new health-care information and techniques to them is recognized by virtually every core work.

The major theme in such research has been the relationship between folk and scientific taxonomies—especially in the domain of livestock diseases, where an in-depth, empirical appreciation of the shape, scope and accuracy of a people's etiological, anatomical, physiological, diagnostic, curative and epidemiological knowledge is essential before developers can even begin to evaluate what, how, and if native veterinary practices should be altered. A considerable number of ERD studies therefore devote attention to trying to sort out and "match up" folk disease identifications and/or taxa with their scientific equivalents (Ba 1982, Grandin 1985, Ibrahim 1984, Ohta 1984, Maliki 1981, McCorkle 1982a, 1983b, Sollod 1983, Sollod *et al.* 1984, Wolfgang 1983, and Wolfgang and Sollod 1986; possibly also *Cabrol 1984 and *Noirtin 1975).

Predictably, this is not an easy task. Medical science classes diseases according to the etiological information afforded by sophisticated laboratory analysis. In contrast, at least pending practical necropsy, folk disease distinctions typically rely on the recognition of morbid signs, more rarely on epidemiology, sometimes on sorcery, or on any combination of these. Moreover, as Ohta (1984) points out, when pathogenic explanations for disease are lacking, it is often difficult to distinguish "disease names" from "terms of symptom" since both may reference morbid signs. Further complicating this picture is the fact that, as among the Twareg of Niger (Wolfgang and Sollod 1986), the same morbid condition may have several appellations depending upon the species afflicted. The result is that a single folk disease category—like *q'icha* 'diarrhea' among the sheep and camelids of the Quechua of Peru (McCorkle 1982a), *wilsere* 'bush disease' among the cattle of the FulBe of Upper Volta/Burkina Faso (Wolfgang 1983), or *azania* 'too much blood' among Twareg camels (Wolfgang and Sollod 1986)—often glosses a wide array of etiologically distinct ailments. Conversely, folk classifications may also assign the scientifically "same" disease to different categories on different occasions, based on varying configurations of the clinical, epidemiological and supernatural information available to the native diagnostician and on the species involved.

Nevertheless, it is clear from these and other studies that pastoral peoples possess a rich store of knowledge about many livestock diseases. To take but one example, Schwabe and Kuojok (1981) describe the extensive appreciation of cattle diseases (and of bovine anatomy and physiology) held by traditional Dinka healers and stockowners. This lore derives from practical experience—e.g., personal observations of clinical signs, sacrificial dissections and specific instances and modes of contagion—coupled with a "rational empirical process" (Schwabe and Kuojok 1981:237) which integrates these and other sources of information. Still, as nearly all researchers of ethnoveterinary epistemology have remarked, some of the resulting folk surmises, explanations and curative or preventive actions are "incorrect in major or minor parts" (Schwabe and Kuojok 1981:237).

Simple semantic and taxonomic investigations can help to pinpoint where stockowners could most benefit from increased etiological and epidemiological infor-

mation, more astute diagnoses, and new treatment, prevention and control options. For the same reason, research into other semantic domains of the animal production system (Anderson 1978, Ba 1982, Flores-Ochoa 1978, Maliki 1981, McCorkle 1983b, Meneses T. in progress) is valuable insofar as many husbandry practices impact upon the occurrence and spread of livestock diseases. Finally, all such research is critical for effective communication between stockowners and development/extension workers. As so many authors have pointed out, the labors of both groups would be eased if they can learn to comprehend and utilize each other's veterinary concepts, techniques and vocabulary.

Ethnoveterinary pharmacology.—This is the investigation of a people's use of plants and other materials in preventing and treating animal diseases, wounds, fractures, in encouraging fertility, appetite, productivity, and so forth. Most core studies make at least mention of this very basic aspect of veterinary care, and some go into considerable detail (Ba 1982:55, 87 ff.; Maliki 1981:47 ff.). Works whose specific focus is the ethnoveterinary pharmacopoeia can range from the folkloristic to the "high tech." Many have an essentially descriptive aim—ie., identification of the materials, their appellation, categorization, acquisition, preparation, indication, administration (including both natural and supernatural operations) and reported efficacy.

Such works may take a purely ethnographic approach. An example is Brisebarre's (1984a) study of the therapeutic use of boquets hung in the sheepfolds of Cevennes, along with her examination of more empirical curative applications of plant and other materials to Cevenol ovines (Brisebarre 1978). Alternatively, descriptive studies may have a more strictly pharmacological end in view, as in Nwude and Ibrahim's (1980) detailing of 92 plant species employed in traditional veterinary medicine in Nigeria for every type of domestic livestock (possibly also *Gourlet 1979). Likewise for Chavunduka's (1976) identification of 53 plant species of ethnoveterinary medicinal importance in southern and eastern Africa, along with their uses, preparation and administration. For veterinary pharmacologists, identification and description are but the first steps toward controlled scientific screening of local plants in order to establish their real utility if any, optimal dosages, and effective frequency of application (e.g. Ibrahim *et al.* 1984, *Mourier-Ballon 1983).

While research of this sort can add useful new drugs to the modern veterinary pharmacopoeia, its ERD importance lies in improved folk pharmacotherapy which is culturally appropriate, economically feasible and consistently available. At this level, its relevance to development and extension is evident. An example is provided by the SR-CRSP/Peru. Building upon existing ethnoveterinary pharmacological knowledge, the project has worked with one peasant community in the central highlands to test the efficacy of a wild tobacco as a botanical for ovine ectoparasites (Bazalar and Arevalo, in progress). As per the longstanding and widespread use of nicotine-based parasiticides in both folk and modern veterinary medicine (Schillhorn van Veen, pers. com.), initial trials have proved successful; and work is now being done to establish the minimum effective compound and to secure supply of the plant (Fernandez 1985). The project also plans to test these tobacco compounds in combination with *tarwi (Lupinus mutabilis)* water. *Tarwi* is a bitter, alkaloid-laden legume which is edible only after prolonged steeping. The trials Bustinza Ch. (1985) performed on this indigenous cultigen's use in southern Peru as a folk cure for ectoparasites of alpaca have already demonstrated its efficacy. Working in conjunction with SR-CRSP social scientists, project veterinarians are conducting similar trials on other plant materials in the ethnopharmacopoeia which are employed to combat ovine endoparasitism (Arevalo and Bazalar, a, b, in progress). Throughout, emphasis is placed on compounds and applications which can be readily prepared and comprehended within the peasant community itself.

Ethnoveterinary manipulative techniques.—This topic is distinguished from pharmacotherapy above and magico-religious procedures below by its primarily mechanical nature—although no such distinctions may be drawn emically. Of course, all these approaches may be used conjointly—as when a stockowner surgically cleans and then sutures a wound, poultices it, and offers up a prayer for the animal's speedy recovery. For convenience, here I lump vaccination and other prophylactic measures with the healing arts—bonesetting, surgery, wound treatment, chiropractic-like manipulations and, at least in China (Metalie 1984), acupuncture. As before, ERD's concern is to identify and describe, discover the emic rationale for, and evaluate the appropriateness and effectiveness of such manipulations.

Ethnoveterinary prophlaxes may be of an essentially empirical, managerial sort, e.g.: smudge fires to drive away disease-bearing pests; manual removal of ticks; avoidance of infested pastures and unclean water; quarantine of contagious individuals; mineral feedings; protection from extremes of weather; and general sanitation measures like cleaning, disinfecting or rotating animal quarters. They may also include various magico-religious performances, taboo observances and so forth (see below). But a more classic example of ethnoprophylaxis is traditional vaccination. For instance, some FulBe vaccinate their cattle against rinderpest by inserting a bit of lung from an infected animal into an incision in the nose, leaving the material in place until the wound festers; others inject a solution in which the lung tissue has been soaked (Wolfgang 1983:58). Fulani (Ba 1982:75) and WoDaaBe (Maliki 1981:60) follow similar procedures for bovine pneumonias. Upon completion of the vaccination process, WoDaaBe also excise the rotting flesh and cauterize the wound.

As a healing art, cauterization appears to be a routine and multi-purpose technique among all Sahelian pastoralists. For example, FulBe treat livestock sprains with a series of tiny burns in the sprained area—much like the "pinfiring" performed on Western racehorses with leg problems, to increase blood flow to the injured part (Wolfgang 1983:57). FulBe, Fulani, Twareg, and WoDaaBe, whether rightly or wrongly, all use branding in treating a galaxy of ills. Across the three ethnic groups, these ills include, e.g.: anthrax, trypanosomiasis, rickettsiosis, epilepsy, edemata, botulism, scabies, bloat, diarrheas, toothaches, fevers, blows to the body, digestive and hoof ailments, muscle pains, sprains and lizard bites. Venesection or bleeding is another popular healing art in African veterinary practice. All of the foregoing authors plus Evans-Pritchard (1969), Ohta (1984), Schwabe and Kuojok (1981), Wolfgang and Sollod (1986) and others note its use.

Bonesetting and wound-treatment skills are found in folk veterinary toolkits worldwide—as are, too, effective surgical and obstetric techniques. These latter run the gamut from relatively simple operations (such as marking, castration, excision of tumors, certain amputations) through a variety of obstetric procedures (e.g., episiotomy, Caesarean section, embryotomy) to complex cosmetic surgery like horn training (Schwabe 1984).

Magic, religion and ethnoveterinary medicine.—This topic has received considerable attention in ERD for a variety of reasons. Admittedly, it is precisely the sort of exotica which anthropologists dote on, and it readily captures the veterinarian's curiosity as well. More importantly, however, magico-religious beliefs and practices appear to form a part of folk veterinary systems everywhere; and in many, emic distinctions between natural and supernatural matters in animal health are blurry.[10] If for no other reason than its pervasiveness, the supernatural must be acknowledged in any ERD study aspiring to a holistic, systems-analysis approach. As an overarching ideological construct, the supernatural can impinge upon every facet of livestock production.[11] However, from an examination of the literature, magic and religion seem to figure most prominently in two areas pertaining to animal health: in the supernatural promotion of livestock

fertility and productivity;[12] and more significantly, in ethnoetiology—which in turn informs folk diagnosis, treatment and prevention of animal disease and accident.

Maliki (1981:65 ff.) presents one of the most thorough-going descriptions of a people's supernatural pastoral repertoire. Writing on the WoDaaBe of Niger, he discusses: fertility, protective and curative rites for animals; hexes, curses and broken taboos which can bring on livestock disease and accident; divination procedures for predicting herd misfortunes; "good and bad luck" days for performing veterinary and other management operations; and more.

McCorkle's (1983b) treatment of these same phenomena for the Peruvian Quechua is equally detailed. However, in addition to describing these Amerind's panoply of supernatural explanations for animal ills, she seeks to analyze them etically. The Quechua etiological category of "evil winds" is illustrative. Indeed, "winds" are common etiologies in a number of folk veterinary systems, including FulBe, Fulani, Twareg and WoDaaBe. This comes as little surprise since certain livestock diseases in fact can be transmitted aerially (e.g., anthrax, foot and mouth disease, rinderpest) and/or promoted by environmental stresses (e.g., a variety of respiratory ailments). Among the Quechua, however, ethnodiagnosis of attack by an evil wind may or may not correspond with any plausible scientific equivalent. Sometimes this diagnosis appears to gloss plant poisoning; sometimes it references a tumorous growth; at still other times, it cannot be linked to any specific clinical signs. Nonetheless, it can often lead to appropriate prophylactic or treatment measures—e.g., keeping animals away from the haunts of evil winds *cum* toxic plants, or surgically removing tumors.

Whether etically translatable or not, as already noted, magico-religious belief and practice figure in folk veterinary systems worldwide, in both developing and developed milieux. To illustrate, Wolfgang (1983) mentions FulBe magical techniques for controlling, avoiding, or curing certain cattle diseases and ethnoetiological agents such as genies. Ibrahim (1984) comments on "spirits" and "the unseen" as explanations among Nigerian Fulani for livestock diseases with unknown (microscopic) causes and neurological signs. Chavunduka (1976:8) notes Manyika tribal beliefs in ancestor spirits and "evil dreams" as origins of disease. "Evil beings" plague Turkana livestock (Ohta 1984). Recurrent themes in Kimball's (forthcoming) observations on Brunei Malay ethnoveterinary practice are magio-mechanical techniques to ward off disease-causing *hantu* spirits, and Islamic prayers such as the "neutralizing harm verse" to forestall various kinds of livestock problems. For Irish stockowners, Shanklin (1985) describes evil-eye theories of animal ills, and their associated ritual and behavioral precautions. Brisebarre (1978, 1984b) and others (cited in Brisebarre 1985b) document a pantheon of French "veterinary saints" to whom provincials still turn to bless, protect, cure and multiply their livestock. And Brisebarre (1985c) analyzes the principles of sympathetic magic behind French stockowners therapeutic use of boquets. Finally, many of the foregoing and other studies (e.g., Schwabe and Kuojok 1981) further indicate what social types of individuals (priests and shamans, sorcerers, herbalists, smiths, heads of household or lineage, wives, etc.) are traditionally responsible for the various supernatural—as well as naturalistic—operations related to animal well-being.

For development and extension, the importance of understanding supernatural aspects of folk veterinary systems is threefold. The most obvious consideration is a diplomatic one. If ERD personnel ignore, belittle, or worse still, unwittingly outrage indigenous ideology, their work is not likely to meet with much success. A second consideration is that sometimes magico-religious practice and idiom in fact embody practical veterinary and management acumen. Treatments like feedings of saint-blessed salt (Brisebarre 1984b) are potentially effective for some maladies; and seemingly *outre* ethnoetiologies like "evil winds" (McCorkle 1982a) can nevertheless dictate appropriate curative or preventive action. Developers must therefore be careful about dismissing

"superstitions" out-of-hand. Third, extension efforts can directly build upon an understanding of the supernatural in folk veterinary systems. Useful management techniques can be reinforced with added information as to how genies, spirits, evil winds/dreams/eyes or what-have-you accomplish their nefarious aims; and new skills can be introduced in a cultural idiom which makes sense to stockowners or at least does not threaten ideological, and related sociostructural, integration.

Ethnoveterinary extension.—With regard to social structure—and as Halpin (1981), McCorkle (1982a), Schwabe and Kuojok (1981), and others have pointed out—one of the most logical choices for recruiting and training effective veterinary extension personnel is local healers who have traditionally dealt with animal (or human) health problems. These specialists or semi-specialists typically share the same language and culture as their clientele; already enjoy their confidence and esteem (albeit to varying degrees); occupy a recognized role in the ethnomedical system; and often control a wide range of empirical medical skills and knowledge. Identifying these individuals, their established domains of practice, their real expertise, and their potential as veterinary extension workers is yet another important task in ERD.

Along the lines proposed for use of traditional healers in human health care in many developing countries (e.g., Dunlop 1975), Schwabe and Kuojok (1981) emphasize that, with some training and organization, such individuals could provide effective and relatively cheap grassroots delivery of basic health services to livestock, and possibly even to humans. Halpin (1981) advises that these "barefoot vets" can be drawn from among stockowners as well as healers. He further notes that a trained coterie of camp-level veterinary extensionists could be particularly effective in nomadic areas, where other types of delivery are so problematic for so many reasons *(cf.* Imperato 1974). In developing nations, these "paravets" could additionally function as a unique component in a "badly needed disease intelligence system" (Schwabe and Kuojok 1981:237) and as accurate interpreters of stockowners' primary veterinary "troubles, constraints, fears and aspirations" (Halpin 1981:5). As these authors point out, such information would in tum permit more rational design, performance and evaluation of livestock disease control programs.

Grandin (1985), Halpin (1981), Loutan (1984), D. Sandford (1981), S. Sandford (1983), Schwabe (1980), and Schwabe and Kuojok (1981) all offer suggestions and observations on how selection, training, supervision, motivation and remuneration, logistics, supplying, reporting and accounting procedures, and etc. of paravets can or has been organized vis-a-vis: multilinguistic realities; complex national government and local social structures—including household, camp, village-chief, interethnic and common-interest group organization; the veterinary worker's specific role within and responsibility to these structures; epidemiological profiles; and animal management and movement patterns. Summarizing the lessons learned from the Niger Range and Livestock Project's pilot paravet program, Loutan (1984) provides a particularly thorough and insightful case study which addresses a majority of these issues.

Animal health and livestock production systems research.—All of the foregoing considerations and topics are implied in this final category, which embodies the core of current ERD. Works in this vein may naturally differ in their topical emphasis and scope, often depending upon the author's disciplinary training and subdisciplinary interests. They may highlight veterinary, management, or socioeconomic and sociocultural aspects of the animal-health and production-system issues examined. They can also vary in their primary, immediate goals of research: thorough-going description, disciplinary theory-building and validation, policy planning, advocacy of a given research design, or investigation of a specific animal-health question. However, all studies in this group share two

defining features: an explicit recognition of the holistic, systemic complexity of the phenomena under study; and an ultimate commitment to making research results useful for livestock development and extension.

Among the first works in this group to reach print is Maliki's (1981) report on WoDaaBe cattle herders in central Niger. The range of topics he treats is indicative of these studies' holistic outlook. To illustrate as briefly as possible, he details: herd composition, dynamics, and ownership and use rights, along with all Fulfulde semantic distinctions in race, sex, age, reproductive and productive state, and personal names for cattle, plus additional categorizations for sheep and camels; every aspect of basic animal management such as pasturing/mineral-feeding/watering patterns and selection/breeding/fertility/gestation/abortion/calving/milking; WoDaaBe description and classification of plants according to their palatability and nutritive value for the different animal species and at different stages of plant growth, plus their veterinary medicinal and other uses; similarly for identification of livestock diseases and other health problems—their ethnoetiology, the clinical signs herders recognize, the specific cures and controls they seek to apply; herd movements during the eight emic seasons of the pastoral year and their impact upon the social groupings and activities of families, camps and clan; relatedly, the role of animals in rites of passage, friend-and kinship, social status and recreation; harvesting, consumption and distribution of all pastoral products; magical beliefs, songs, proverbs, origin myths and etc. pertaining to herds; and still more—all with precise transcriptions of the hundreds of lexemes in the WoDaaBe herding vocabulary. Ba's (1982) treatise on the "veterinary arts" among Sahelian Fulani (Peul) follows a similar format, but with a tighter focus on veterinary and related management practices, and a more limited discussion of social, economic and cultural correlates of Fulani animal husbandry. Both studies are essentially descriptive.

McCorkle (1983b) covers largely the same topics as Maliki—plus others such as the social organization of labor for herding (1982b), and management issues in sheltering, shearing, docking, castration and predator control (1983a)—for Quechua Indians of Peru. However, she has a theoretical as well as a descriptive aim: to correct neofunctionalist analyses of agropastoral subsistence systems. Using a New World data base to refine and validate the cross-cultural applicability of a "dialectic" model of preindustrial agropastoralism in Europe, she demonstrates how Andean herding and cropping stand in a simultaneously complementary and competitive relationship to each other. In the process, she outlines how veterinary care, in particular, is constrained by the low productivity and multiple competing demands of paleotechnic agriculture. Under their present "meat and potatoes" production system, this leaves Andean peasants short of land, labor, capital, technology and technical information for significantly increased attention to herd health problems—certainly insofar as intensive, costly, "tech-fix" solutions derived from Western commercial practice are concerned. For livestock development and extension, McCorkle further discusses some of the systemic potentials and problems posed by ecological, sociostructural, and sociopolitical factors relating to, eg., communal land tenure and pasture/field usufruct rules, traditional reciprocal labor patterns and centuries-old ethnic dominical mechanisms. The ultimate implication for livestock development is that only a global, systems analysis which acknowledges the dialectical tensions between preindustrial cropping and herding can forestall the error of "robbing Peter to pay Paul"—i.e., of upping pastoral production at the expense of agriculture, or vice versa.

The paramount concern of Wolfgang's (1983) work among the FulBe of west-central Upper Volta is to arrive at specific recommendations for veterinary extension and policy planning. To this end, she focuses her research on three major areas: (1) FulBe classifications, etiologies and treatments (both folk and Western) for cattle diseases, plus herders' own assessment of the socioeconomic impact of different diseases; (2) the

current structure and functioning of animal health-care delivery services in the region; and (3) a survey of the country's major veterinary diagnostic laboratory facilities. Additional topics of investigation include certain non-disease-related health problems of cattle and (especially in Sollod *et al.* 1984) women's role in maintaining herd well-being. Findings from all these areas inform Wolfgang's final recommendations for veterinary extension and policy in Upper Volta (now Burkina Faso).

A sampling of these recommendations is of interest because they reflect needs common to many developing countries. One is an immediate improvement in epidemiological information, so that planners can concentrate scarce resources on the most prevalent, economically damaging livestock diseases. Another is educational outreach to correct folk misunderstandings about and consequent misuses of expensive Western drugs. A closely related concern is to remove communication, and even simple translation, barriers between stockowners and extension agents—a problem which, theoretically at least, could be resolved by incorporating some herdsmen into the livestock service, as has been done in other parts of Africa. Finally, Wolfgang notes a need for modest improvements in regional laboratory diagnostic facilities, and in other technological and infrastructural aspects of health care delivery. Throughout, however, she emphasizes that including stockraisers themselves—both women and men—as substantive participants in the extension process should greatly enhance diagnostic, delivery, and treatment effectiveness and cost-efficiency.

Sollod *et al.*'s (1984) aim is somewhat more didactic and programmatic than that of the foregoing studies. These authors seek, first, to define and codify the exciting new trend in ERD which tackles animal health and production system research through "veterinary anthropology." Then, drawing upon the fieldwork of Sollod and Knight (1983) (a veterinarian and an anthropologist) among herding groups of central Niger, plus Wolfgang's investigations (which were in part supervised by Sollod), they demonstrate how this fusion of perspectives and methodologies can greatly enrich analyses of patterns, problems, and control options in livestock health.

Sollod and Knight's Niger research is particularly instructive. There, the interdisciplinary team was able to relate epidemiological profiles of livestock diseases—their incidence, prevalence, seasonality and geographic distribution—directly to differing systems of animal production (Twareg versus WoDaaBe) and to specific husbandry practices within these systems which promote or discourage expression of a given ailment. These practices, in turn, were linked to concrete ecological, cultural, commercial and subsistence parameters of Twareg and WoDaaBe life. For example, it was found that stress-related pneumonia and protein-caloric inanition were severe problems among WoDaaBe, but not Twareg, sheep. This finding was related to the seasonal timing of ovine births. The Twareg control breeding through penile sheath ligation of rams, thus ensuring that ewes do not give birth towards the end of the dry season, when forage is scarce and nutrition poor. In contrast, the WoDaaBe—who consider themselves to be cattle herders—expend little effort of any sort on their sheep. The only "control" they exercise on breeding is sales of rams in response to market demands for mutton, especially at the time of the annual Id festival. Depending on whether this moveable feast falls before or after the first breeding season, WoDaaBe ewes and their lambs suffer or thrive accordingly (after Sollod *et al.* 1984:291).

The veterinary anthropology which these authors espouse highlights the dynamic interplay of endogenous and exogenous determinants of disease—the latter defined as factors external to etiological agents or their hosts. The contextualized, culture-specific information which this comparative stance yields is critical for the design of successful development and extension programs because "It makes possible the use of nonmedical approaches to animal health which include marketing and management interventions, and allows the use of a simplified package of veterinary commodities for each pro-

duction system" (Sollod *et al.* 1984:292). It has long been recognized, and repeatedly demonstrated, that changes in management alone are sufficient to control many livestock diseases. Yet as Schillhorn van Veen (1984:306-308) has observed, despite the fact that such interventions can be highly beneficial at relatively low cost and risk, management is rarely defined for indigenous stock operations. The interdisciplinary, holistic and production-systems approach of veterinary anthropology works to fill this definitional and empirical lacuna.

Finally, by virtue of its holistic, production-systems orientation, Shanklin's (1985a) work among farmer-stockowners of northern Ireland also falls in this last group of studies. The ethnoveterinary portion of her monograph is designed to test a single hypothesis: "that if different types of animals are kept in a given environment, susceptibility to disease will be a factor in the decision to keep a specific type of animal and selective breeding will be largely determined by this consideration" (Shanklin 1985a:215). To this end, she marshals comparative data on bovine as versus ovine production with regard to: stabling, pasturing, seasonal supplemental feeding, both folk and scientific veterinary knowledge and care, breeding practices, land and labor requirements, economic value and market outlets, government regulation and historical shifts in these and other production parameters. Her larger aim is to review theoretical debates in ecological anthropology relating to the adaptive value of traditional and non-traditional elements in the animal production system, and to identify ecological constraints to indefinitely upping livestock production.

CONCLUSIONS

ERD is still in its infancy—or perhaps with the appearance of conceptually and disciplinarily more integrative papers like Schillhorn van Veen (1981, 1984), Sollod *et al.* (1984), and the present review—its early adolescence. As is to be expected of a young area of research, many ERD works are still focused on the descriptive level; and across the field as a whole, there is a healthy diversity of topics and approaches. Again, where this diversity finds a unifying form and function, however, is in international livestock development. Here, ERD is of critical importance because without improvements in animal health (and nutrition), rarely can any improvements in livestock productivity be achieved. In response to this need, a contemporary core of development-oriented ethnoveterinary research has emerged.

Within this core, a number of shared themes, methodologies, and perceived needs for future research can be distinguished. First and most salient, of course, is an emphasis on the "ethno" in ERD. As recognized for other development sectors *(cf.* Brokensha *et al.* 1980), a thorough-going understanding of and respect for folk veterinary knowledge, concepts, practice and practitioners is a must. While clearly not all elements of ethnoveterinary (and their associated management, sociostructural, and etc.) systems are accurate or effective, their ensemble represents a rich resource for developers seeking to enhance animal health and productivity in ways which are readily comprehensible and culturally acceptable to the client audience and which are ecologically and socioeconomically sound. In other words, existing folk practice and belief should always be the starting point for veterinary research, development and extension—as, indeed, they were in the evolution of Western veterinary medicine.

Second, as we have just seen, there is an invigorating move in ERD towards analyzing veterinary development issues within a holistic but comparative and production-systems-specific framework. Production systems or subsystems may be defined by culture area, ethnic group, agroeconomic sectors (e.g. cropping versus herding, sale versus subsistence), intraethnic household characteristics, species or other parameters like ecozone. This new dynamic in ERD has brought with it an explicit recognition that

the constellation of endogenous and exogenous variables impinging upon animal well-being ultimately lies beyond the ken of any one technical or social science. Ideally, research into the complex, real-world coordinates of livestock health should therefore be carried out by concerted interdisciplinary action. Veterinarians and anthropologists have together risen to this challenge; and there is both room and need for collaboration with other disciplines, as well. Such research naturally calls for in-depth field-based studies rather than just laboratory analyses "divorced from the realities of pastoralism" (Sollod *et al.* 1984:285). In this regard, the usefulness of time-tested methods of anthropological fieldwork is undisputed in ERD. Likewise for the ethnographic expertise and the emic, bottom-up perspective of anthropology.

In the findings and hypotheses of animal health and production systems research to date, some consensus on development and extension strategies is also emerging. To wit, that educational, managerial, marketing, and other such interventions may often prove more appropriate, economical, and effective than modern drug therapy, eg., as applied in mass vaccination and treatment schemes. In the rush to implement costly top-down, "tech-fix" programs which offer immediate short- term benefits, developers, policy planners and stockowners alike may lose sight of longer-term drawbacks to such solutions in third-world countries. These drawbacks can include: ecological degradation and depletion, as from overgrazing; relatedly, escalating social and political tensions over competition for scarce feed and water; spasmodic breakdowns in veterinary supply and delivery lines due to an unstable economy and/or government, or to infrastructural inadequacies; political and financial machinations within the livestock service; loss of genetic tolerance to disease in stock, and increasing drug resistance in vectors and etiological agents; and more. (For an interesting case study of some of these problems, see Lawrence *et al.* 1980.) There is also agreement in core ERD on the wisdom of employing local healers and stockowners themselves as extension agents or assistants, although equally it is recognized that their use is not problem-free and requires careful selection and organization.

As for future research needs, there is a clear consensus on the vital necessity of everywhere acquiring more, and more accurate, epidemiological data—data which must be collected, compared and analyzed in both emic and etic terms. This very basic sort of information is obviously imperative if valid correlations are to be drawn between patterns of livestock disease and the physical and human ecologies which animals and their keepers inhabit. It is also imperative for meaningful communication between stockowners and ERD personnel.

Beyond the need for improved epidemiological information, I would like to add several other areas which I perceive as requiring more attention. One is the formal, ethnoscientific study of folk classifications for livestock diseases/etiologies/cures, types of pastures and rangelands, species and races of animals, and so forth. To the best of my knowledge, ERD investigations of ethnobiological categories have so far been carried out largely by individuals inexpert in the rigorous procedures of formal linguistic analysis. Yet such analyses, we know, can reveal not only the underlying logic of folk conceptual systems, but sometimes also crucial biological and sociological facts and interrelationships overlooked by Western science. This untapped source of systematic information could be of great potential value to ERD because, as one author has convincingly argued, in some cases folk practice or conceptualization of a problem may prove comparable or superior to that of established science. In others, the two perspectives may diverge but may both embody important insights which can be synthesized. In either case, it is desirable to transcend the conventional science/indigenous, active/passive dichotomy to allow greater indigenous participation in determining development goals and means (after Howes 1980:342). Formal ethnosemantic analysis has an obvious role to play in this discovery process.

Another area which has received surprisingly little attention is the many parallels between human and animal ethnomedical systems. The vast literature on human traditional medicine (see, eg., Harrison and Cosminsky 1976) rarely mentions any link between the two. Yet the ethnoveterinary literature contains repeated hints that they are not always highly differentiated. Indeed, as Schwabe and Kuojok (1981) and Schwabe (1978) observe, knowledge derived from folk veterinary experience may be analogy inform human ethnomedical concepts and practice. Moreover, *Homo sapiens* and their domestic animals share many ills; and livestock often serve as hosts, reservoirs, vectors and agents of human disease (*cf.* Schwabe 1969). As these authors point out, the links—both folk and scientific, direct and indirect—between human and animal medicine suggest the possibility of mounting coordinated programs of health care. This is a particularly attractive development and extension potential for remote, nomadic, poor, or otherwise ill-serviced regions.

Finally, I suggest that it is time at least to begin substantively integrating and theoretically synthesizing ERD findings to date. An overview of the literature reveals many commonalities—and even some startling identities—in folk veterinary beliefs and practices cross-culturally. Unfortunately, there is not space in this review to launch a discussion of these congruencies and their probable causes. Clearly, though, both the similarities and differences in ethnoveterinary systems worldwide need to be catalogued, systematically compared with their correlates in human ethnomedicine and Western veterinary science, and explained.

In arriving at larger explanatory models of ethnoveterinary phenomena, the relatively more advanced field of medical anthropology holds forth some pertinent analytic frameworks. As noted above, folk medical theory and practice for animals is both emically and etically related to that for humans. Consequently, general research topics and approaches in medical anthropology and ERD frequently overlap. (For a survey of medical anthropology concerns see, eg., Colson and Selby 1974, Foster and Gallatin Anderson 1978, McElroy and Townsend 1979.) Again, there is neither the space, nor perhaps the need, to detail these touchpoints here. Suffice it to reiterate that "veterinary anthropology" can profit from much of the analytic groundwork already laid in its sister field of medical anthropology.

Likewise for programs of veterinary extension vis-a-vis social science models of cultural change and development, theories of innovation and modernization, the FSR&E literature (farming systems research and extension), and communications theory. As ERD begins to compile and integrate its holistic knowledge of folk veterinary medicine in a production-systems context and to apply itself to hands-on extension, perspectives derived from these cognate areas of research can do much to insure that its insights into the real-world complexity of ethnoveterinary systems are appropriately and effectively utilized. These analytic and synthetic tasks now facing ERD offer an even newer direction for this "new direction in ethnobiology."

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Lettre of the Societe d'Ethnozootechnie, and other periodicals. Finally, thanks are due the *Journal of Ethnobiology* for providing the stimulus to this review, which has brought many ERD workers into contact for the first time.

NOTES

[1]Although it avoids the academically hienous mixing of Latin and Greek roots, "ethnozootechnics" is perahps a too-narrow term. It could be taken to imply the study of folk veterinary knowledge and technique to the exclusion of larger considerations (ideological, socio-organizations, economic, etc.) which also influence the management of animal health. "Veterinary anthropology," while linguistically inelegant, obviates this problem. As "the study of 'man' from a veterinary viewpoint," it focuses attention upon the importance of animal health and productivity for *human* well-being rather than as decontextualized ends in and of themselves. Moreover, it precisely captures the core of inquiry at the forefront of contemporary ERD (see text). And, it forms a nice analogy to "medical anthropology," since ERD in many ways parallels for animals this domain of study among human populations. Also, the term is innately appealing to anthropologists like myself who work in this area. Still, a slightly less disciplinary-specific label might be more indicative of the field in its broadest definition and antecedent forms—and hence, too, more politic. So despite its Latin-Greek mix, here I employ the overarching "ethnoveterinary" (McCorkle 1982a) to reference the field as a whole.

[2]This is most likely where the agroeconomic base or, better still, the animal production system itself forms one of the foci of research. To give but a few yet representative ethnographic examples, in an extensive study of Saami ethnoecology, Anderson (1978) details both past and present systems of reindeer management, their sociostructural correlates and physiographic setting, and touches upon *Rangifer* nutrition and health. Evans-Pritchards' (1937, 1969) classic investigations among the Nuer document many health-related aspects of their cattle husbandry, although unfortunately he mentions little of Nuer veterinary medicine *per se*. And works like those by Flores-Ochoa (1979) and West (1981) on alpaca herders in Peru, Bernus (1981) and Nicolaisen (1963) on the Twareg, Okaiyeto (1980) and Stenning (1959) on the Fulani, Dyson-Hudson and McCabe (1985) on the Turkana, and many others offer occasional observations on the types and occurrence of animal diseases, health-related management practices, contact points between folk and modern veterinary science, and so forth.

A patchwork of ethnoveterinary information is also tucked away in the fieldnotes and "heads" of ethnological and archaeological researchers. This is illustrated in personal communications from: Lynn Hirschkind, for a variety of animal domesticates in Ecuador; Frank Hole, on reproductive, ethnoetiological, ethnodiagnostic, and other aspects of sheep and goat husbandry among the Lur of the Zagros mountains of Iran; Joel Knipers, for folk theories of equine health in eastern Indonesia; and David Lonergan, on the veterinary beliefs and practices of shepherds in central Sardinia.

[3]Snippets of ethnoveterinary lore may appear in works that touch upon witchcraft, ritual, and religion as these relate to health and healing (e.g., Buxton 1973 and Richards 1927, cited in Schwabe and Kuojok 1981; *Jalby 1974) or in addenda to discussions of human ethnomedical systems (e.g., the appendix to Hockings 1980).

[4]For example, researchers in a variety of disciplines have sought to reconstruct elements of veterinary knowledge and technique among ancient peoples (e.g., Bodson 1984, Roquet 1984, Schwabe *et al.* 1982, Schwabe 1978, 1984). Also, recent archaeozoological work provides some insights into disease patterns (e.g., Wheeler 1984) and stresses (e.g., Pollard and Drew 1975) among early animal domesticates.

[5]Such studies may allude to folk beliefs or, more typically, disease-related husbandry practices, e.g.: in veterinary medicine, Fazil and Hofmann 1981; Higgins 1983; Reed *et al.* 1974; Schillhorn van Veen 1981, 1984; Schneider 1977; Sollod 1981; and in range science, Glazier 1982.

[6]The extensive historiography of Western veterinary medicine and its practitioners documents much of the discipline's folk underpinnings (e.g., Smithcors 1957, and many others). And specialized works on a given animal domesticate (e.g., Law 1980) sometimes mention ethnoveterinary techniques and theories applied to that species.

[7]An exhaustive review was the initial ideal, but this was thwarted by a number of factors. For one, the limited number of researchers whom I have been able to identify as working specifically in ERD is flung 'round the world—most notably in the U.S., Europe, and Africa. Moreover, we appear to have been only partially aware of one anothers' work, especially when we step outside our primary geographic area(s) of research. There as yet exists no formal network, or even an informal community, of ERD-ers. Nor is there any recognized group of journals in which ethnoveterinary information regularly appears. Indeed, a good deal of the ERD literature exists only in "fugitive" form: in unpublished mimeo or xerox, in recondite newsletters and journals, in USAID and other project technical reports, in theses and dissertations from third- as well as first- and second-world countries—and of course, all in a variety of languages.

Sifting through the literature that is available presents yet another basic problem. Because, as noted earlier, there is no one label for the field, from titles alone it is often impossible to distinguish between works with and without an ethnoveterinary orientation. Of course, titles which contrapose "veterinary" with "anthropology," "ethno," "traditional," "indigenous," "popular," or "folk" pose no problem. But more amorphous appellations like "Epidemiology of Animal Disease X in Place X" or "Herding Among the X People" may or may not have an "ethno" and a veterinary component, respectively. Each such work must be carefully examined for its perspective and content.

[8]Among the earliest and most sustained of such efforts are those of French researchers investigating the folk veterinary medicine, both past and present, of France (e.g., Societe d'Ethnozootechnie 1984). Brisebarre (1985b) has compiled a thorough-going annotated bibliography of 57 works dealing in part or in whole with this topic. It is not feasible to reproduce all these listings in the space available here. In any case, many offer only piecemeal ethnoveterinay observations; and only a few of the remainder were available for firsthand examination. Nevertheless, based on their titles and annotations, a number of these publications are clearly relevant to sections of the discussion. In such instances, these studies are cited with an asterisk. Finally, Brisebarre (1985a) has also produced a companion, but unannotated, bibliography listing 63 theses in veterinary medicine produced in France between 1970 and 1984 which deal with pastoral research in Africa.

[9]"Ethnosemantics" is employed here in a simple, non-technical sense. I do not mean to reference the formal linguistic discipline known variously as ethnosemantics, ethnoscience, or componential analysis. See concluding remarks.

[10]For contemporary Nilotic cattle-culture peoples, Schwabe (1984:140) remarks that "the practices of animal husbandry, religion and healing are thoroughly mixed." Maliki (1981:54) notes that "there is a thin line between" pharmaceutical and magical veterinary treatments among WoDaaBe. McCorkle (1982a:7) writes that Quechua villagers make little or no distinction between natural and supernatural ills and cures.

[11]For concrete examples of ideology-based impacts upon other management activities like culling, slaughter, marketing, restocking, pasturing, breeding, docking, and predator control, see McCorkle 1983a, b.

[12]Detailed descriptions and symbolic analyses of livestock "fertility rites" are fairly abundant in the anthropological literature. However, insofar as these accounts fail to link such rites to any larger issues in animal health and husbandry, I do not reference them here.

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GENETIC DIVERSITY AND CONSERVATION IN TRADITIONAL FARMING SYSTEMS[1]

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ABSTRACT.—A few farming systems have carried forth the legacy of Neolithic in the thousands of ancestral crop varieties known as "landraces." Our debt to this heritage is immeasurable, yet the very success of modern agriculture is contributing to the disappearance of landraces. A handful of scientifically bred varieties has replaced thousands of ancestral types in twenty years as part of the worldwide effort to develop agriculture. Although landraces and the farming systems that maintain them are part of long evolutionary processes, rapid change threatens their survival. Given the value of landraces for genetic resources, their replacement has been termed "genetic erosion."

This paper develops a framework for analyzing genetic erosion and discusses the status of genetic resources of crops in two areas of genetic diversity, the Andes and Southeast Asia. Similarities and differences in the pattern of genetic change relating to the adoption of modern crop varieties are observed. The pattern shows a dichotomy between commercial and subsistence farming systems and between micro-regions. By describing conservation and loss of genetic resources we can obtain a more accurate understanding of agricultural change in underdeveloped and marginal areas.

GENETIC EROSION

Genetic erosion refers to the loss of genetic resources because of human activity. It occurs in "natural" ecosystems, for instance with the clearing of tropical forests, as well as anthropogenic systems, for instance when ancient landraces of crops are replaced by modern varieties. Concern for genetic erosion arose with the recognition that biological diversity is being lost at an unprecedented rate because of the rapid demographic, economic and technological changes of the late Twentieth Century (Myers 1981). This paper treats the loss of germplasm from traditional farming systems.

In thinking about genetic erosion, we confront the familiar issue of environmental costs resulting from economic growth. The value of genetic resources, particularly crop germplasm, is undisputed. Crop germplasm is a renewable resource, but agricultural change in areas of genetic diversity may involve replacing one type of germplasm ("landraces") with another ("modern varieties"). This replacement destroys the older type, unless it is collected for storage in germplasm collections of seed banks. Two issues surround this replacement. One is the efficacy of conservation outside of living agroecosystems (Frankel and Soule 1981). The other is the issue of environmental degradation in ancient agricultural systems that existed for millennia without external subsidies. Although increases in productivity can be gained through germplasm substitution, they are usually accompanied by increased management costs and reliance on purchased inputs, especially fertilizer and seed. Genetic diversity in agriculture is also theoretically associated with stability (VanderPlank 1968), and a decrease in diversity thus represents environmental degradation to a less stable system. However, the diversity/stability argument has not been adequately investigated in relation to agriculture, and contrary views are prevalent (e.g. Brown 1983, Altieri, *et al.* 1983).

In the early 1970s, two phenomena coincided to change fundamentally the way we think about the genetic resources for agriculture. First was the recognition that the "Green Revolution" was taking hold in many parts of the world, especially Asia. Predictions of famine that were common in the 1960s were replaced by concerns over the impact of the abundant harvests from the high yielding varieties (HYVs) of rice and wheat (e.g. Wharton 1969). The Green Revolution was succeeding in areas of genetic diversity which are the primary providers of the genetic resources for agriculture everywhere. The success of HYVs showed us that "traditional" cultures were rapidly changing. Second was the spread of the southern Corn Leaf Blight through maize fields of the southeastern United States. This lead us to recognize how vulnerable modern agriculture is to exotic pathogens, owing to its reliance on a narrow genetic base (National Academy of Science 1972). The context for this coincidence included the ideas of a population bomb about to explode, the finite nature of resources, the OPEC oil shock of 1973, and the environmental movement's notion of the "Spaceship Earth" and its celebration of traditional cultures.

This coincidence linked two popular notions: the fragility of modern technology and lack of control over a base of critical resources. The benefits for poor people in underdeveloped countries reaped from the Green Revolution represented a "hidden peril" for agriculture: the loss of genetic resources that serve to protect crops from pests and pathogens (Chedd 1970). The term "genetic erosion" represents the wedding of these notions, implying a limited resource base being destroyed by peasant farmers as they modernize.

The response to this perceived peril was to increase the collection and conservation of genetic resources. The first half of the 1970s produced landmark studies of the status of genetic resources in agriculture (e.g. Frankel and Bennett 1970; Frankel and Hawkes 1975). National germplasm systems were strengthened, and an international system for monitoring, collecting, conserving, and distributing genetic resources was organized in various ways, including establishing the International Board for Plant Genetic Resources (IBPGR) (Plucknett and Smith 1982). A systematic program was planned for the world's major food crops, to set priorities for collection and to evaluate the status of germplasm collections.

The growth/conservation debate continues, and it is apparent that information on the nature, status and trend of genetic erosion is much needed (U.S. Strategy Conference 1981). Research is required for policy and planning, whether to accelerate collection or to develop *in situ* conservation. The use of ethnobiological research tools has great potential for studying the dynamics of genetic resources from ancient farming systems. These resources are cultural as well as biological objects, and selection is a function of social, economic, and cultural factors as well as a function of the germplasm's biological characteristics.

THE ORIGINS OF CROP GENETIC DIVERSITY

Centers of crop origin were first recognized by their high levels of genetic diversity, both at the species and variety level (Vavilov 1951). Crop germplasm from these centers of diversity forms the foundation of the crop breeding industry that is essential to modern agriculture, and thousands of cultivars have been observed and collected from these centers. Research on the geographic origins of agriculture (Ucko and Dimbleby 1969) indicates a climatic prerequisite of seasonal variability and a well-marked dry season (Hawkes 1983), and it showed that mountainous or hill land areas were especially significant.

Four general factors can be cited in reference to this diversity: (1) the physical diversity of centers of origin, (2) the long history of cultivation, (3) the large number of coevolved

pests, pathogens, and competitors, and (4) cultural practices of selection, maintenance and distribution. These factors have created numerous niches for distinct cultivars, isolating mechanisms, and selection pressure for diversity. Ethnobotanical research shows that all farming cultures classify and select plants according to many criteria: agronomic, culinary, medicinal, ritual. The rich lexical domains associated with traditional agriculture in areas of domestication are indicative of the positive role that farmers have played in selecting and maintaining genetic resources (Brush *et al.* 1981).

Diversity appears to be one of several strategies used to create stable subsistence by farmers practicing low-energy, non-industrial farming. Other strategies include terracing and other slope modification, mulching and other soil amendments, and frequent field rotation with long fallow periods. Diversity is manifested in low-energy farming systems in numerous ways: in the mix of cultivation, gathering, and grazing as productive activities, in nutrition strategies (DeWalt 1983) in polyculture whereby different crops are produced in association with one another (Kass 1978), in the possession and cultivation of numerous small plots (Carter and Mamani 1983), in the agronomic technologies of field preparation and irrigation (Wilken 1972), and in large numbers of cultivars of single species (Brush *et al.* 1981).

Genetic diversity in crops continues in many environments, but its continuity is particularly marked in mountain areas. Genetic diversity continues here for some of the same reasons that made hill lands centers of crop domestication: well-marked environmental change over small distances, marked seasonality, and isolation between small production zones. Equally significant, however, is the relative marginality of mountain regions in world agriculture. This marginality is the result of numerous factors: lower population densities, greater environmental risk, less accessibility to markets, limited arable land, and lower investment in agricultural development.

These environmental and economic factors have retarded the pace of change, including the substitution of improved crop varieties common to lowland areas. Mountain areas are, however, under considerable pressure to change. Population growth, economic development, greater communication, and political reform stimulate change, leading to some alarm over potential environmental degradation in the highlands (Messerli 1983). Well-tuned traditional agricultural practices, such as crop diversity, terracing, small irrigation systems, and long fallow periods, are being replaced by more intensive, and perhaps less stable practices.

While genetic diversity is inherited from ancient agricultural traditions and may be explained as a response to a complex and competitive environment, it is also associated with low productivity. Jennings and Cock (1978) show that crop productivity in centers of origin is appreciably lower than outside these centers. This is true even after economic and cultural differences are taken into consideration. Agricultural development for greater productivity requires increased management and control over the various components of the farming system. Genetic control and improvement has been accomplished by national and international breeding and seed multiplication programs, exemplified by the release of HYVs of rice and wheat. Breeding and multiplication programs have been developed in most underdeveloped regions since World War II, and by the early 1960s the results of these programs were evident. Farmers were beginning to adopt HYVs in large numbers, leading to inquiries about the rate pattern of adoption (Perrin and Winkelman 1976). Concern about whether new technology could or would be adopted on small as well as large farms and concern about negative impacts of adoption lead to an extensive research effort in the social sciences (Feder *et al.* 1982). This effort revealed that adoption cannot be predicted by the scale of the farming enterprise and that negative influences such as decreased labor demand in agriculture do not result. It also revealed that a number of factors in the general agricultural economy influence adoption. These include risk aversion (Roumasset 1976), human capital (Evenson 1973), labor availability

(Norman 1969), credit (Lipton 1976), tenure (Parthasarathy and Prasad 1978), and the availability of complementary components such as fertilizer and water (Clay 1975).

This social science research on the adoption of HYVs also revealed that the pattern and rate of adoption was very uneven across geographic regions. Following the classic analysis of hybrid corn diffusion in the U.S. by Griliches (1957), a sigmoid pattern of adoption of HYVs in the Third World is evident (e.g. Herdt and Capule 1983; Dalrymple 1978). The rate and ceiling of adoption is variable. Adoption in some places proceeds rapidly toward 100% of farms, while in others it proceeds slowly to a much smaller percentage. The implications of different rates and ceilings of adoption are very significant for our understanding of genetic erosion. They suggest that simple extrapolation from early adoption experience may not be valid to describe the eventual pattern of genetic replacement of traditional varieties of HYVs. In order to elucidate this conclusion, it will be helpful to review two cases of the diffusion of improved varieties. these are the cases of potatoes in the Andes and of rice in Southeast Asia.

SELECTION FOR DIVERSITY: TWO CASE STUDIES

Andean Potato Agriculture.—The potato (*Solanum* spp.) is the staple in the Andes, where it was domesticated. Seven species are cultivated from four ploidy levels (Hawkes 1978). Diversity at the species level distinguishes bitter species (*S.* x *juzepczukii*, *S.* x *curtilobum* and *S. ajanhuiri*) from non-bitter species (*S. tuberosum*, *S.* x *chaucha*, *S. stenotomum* and *S. goniocalyx*). The former are frost resistant and must be processed by freeze drying (into *chuño*) before consumption. Besides species level diversity, the Andes is also a region of tremendous varietal diversity in the potato. The International Potato Center has a collection of some 12,000 named accessions, representing roughly 5,000 distinct clones (A. Huamán, personal communication). Andean farmers employ a systematic folk taxonomy to identify and select varieties (Brush *et al.* 1981). It is common for farmers to be able to identify 40 different varieties, and single fields may be planted in as many varieties. The size of household inventories of potato varieties and amount of field diversity differ greatly between households and regions.

Andean potato agriculture is diverse in many ways besides the number of species and varieties under cultivation. The management of potato farmland is frequently under simultaneous control by households and communities. Households are responsible for selecting varieties, for most of the labor employed in potato cultivation, and for post-harvest processing, storage and marketing. The community controls sectoral fallowing which determines where the household will grow its crop; it regulates the agricultural calendar; and, in some cases, community-based irrigation is used for potatoes. As in all potato agriculture, the maintenance of viable seed is crucial. This depends on the regular rotation of seed tubers between ecological (altitudinal) zones. This sytem may be managed at the household level by the ownership and use of different fields at various altitudes or by communities and market systems that move large quantities of seed across wide regions. Traditional potato agriculture is also associated with a highly fragmented land-holding system. Carter and Mamani (1983), for instance, describe a farming community in Bolivia in which the average household owns 21 different plots in the potato zones. Tillage techniques (Tapia 1983) and post harvest technology (Werge 1979) are similarly varied.

The same forces that contribute to agricultural change elsewhere are felt in the Andes. Population has grown at over 2.5%/year for three decades, popular pressure for development and increased living standards are evident, reform has been undertaken to stimulate agricultural change, and an agricultural research and development infrastructure has been created to promote change. Urban demand has grown in the highlands stimulating the growth of market systems (Appleby 1976).

Highland potato agriculture in Peru has been the object of development efforts for over three decades. Improved varieties from native germplasm were first released in the early 1950s, and since then over 30 varieties have been released. These are now ubiquitous throughout Peru. Besides the release of improved varieties, the Peruvian National Potato Program has promoted the diffusion of selected native clones and encouraged the adoption of modern techniques such as the application of chemical fertilizers and pesticides. The availability of credit for these agrochemical inputs has been tied to the adoption of improved varieties. Three decades of these development efforts have had a clear impact in the Andes; the use of agrochemicals is common, improved varieties have been adopted to some extent almost everywhere, and the infrastructure to promote these changes (markets, roads, extension services, seed production systems) has been expanded.

The extent of these changes has led to predictions of serious genetic erosion in the Andes (Ochoa 1975), and some of the limited data available from the region indicates that these predictions are valid. Research in the Mantaro Valley, for instance, indicates that roughly 65% of the potato land is planted in improved varieties (Horton 1984). This valley, however, is not typical of most of the highlands, being one of the most commercial and intensively cultivated anywhere in highland Peru. The poor performance of Andean agriculture (Caballero 1981; Gonzales 1984) suggests that the overall adoption of new technology, including improved varieties, is low. My impression supports this conclusion, but systematic data on the use of improved and traditional varieties are insufficient to calculate regional or national rates of the use of different types of potato. Horton (1984) reports high adoption rates for well developed and important commercial production areas, but these rates should not be extrapolated over the general Andean region. Bidegaray and Schmidt (1985) report a low 25% adoption rates in the Cuzco area.

Changes, including genetic erosion, are evident everywhere in highland potato growing regions of Peru, but their distribution is very uneven. In some areas diverse native collections have been entirely replaced by improved varieties, while in others native types continue to dominate. The distribution of this change follows socioeconomic and environmental contours. The greatest loss has occurred in lower potato growing zones and those within major valley systems with urban centers and markets. The least genetic erosion occurs in higher zones more distant from urban centers and markets. Research conducted by the International Potato Center (CIP) and by Brush, Mayer and Fonseca, indicates that the adoption of improved potato varieties is only one of several possible options open to small farmers, and it may not be the first option taken to increase production.

CIP studied three agroclimatic zones, based on altitude, in the Mantaro Valley of central Peru. The lowest zone (3000-3450 m) is adjacent to the major urban and market centers of the valley. Although small farms are most typical, this zone has more medium and large scale farms than the other zones, and these types produce potatoes for commercial distribution. The mid-altitude (3450-3950 m) and high latitude zones (3950-4200 m) have fewer commercially oriented potato farms, and potatoes are generally more important as a crop. Farms in all three zones share a number of characteristics: use of agrochemicals, marketing of a portion of the crop, and reliance on off-farm employment to supplement farm income. One of the most striking differences between these agroclimatic zones is in the adoption of improved varieties. Franco *et al.* (1979) report that 87% of fields of all farms in the low zone were planted in these varieties. This reliance drops sharply as one crosses into higher zones. In the intermediate zone roughly half of the fields were planted in improved varieties, while in the highest zone, the rate drops to 12%. Other research indicates that farmers in the high zone interplant improved and native varieties and frequently reserve one or more fields for mixed native varieties alone (Brush *et al.* 1981).

The distribution of innovations in potato agriculture that is found in the Mantaro Valley appears to be typical of most of the Peruvian highlands. Current research being conducted by Brush, Mayer, and Fonseca along the eastern slopes of the Andes corroborates this. In the Tulumayo Valley, east of CIP's Mantaro study area, improved varieties and selected native varieties are grown for commercial purposes in both high and mid-altitude zones. Mixed fields of native varieties are retained under traditional tillage practices, although they are grown in only the highest fields. The total area devoted to native varieties is smaller, and these are grown under greater environmental stress than previously. In the Paucartambo Valley, east of Cuzco, improved potato varieties have made less impact, but much land once grown in native potato varieties has been converted to barley as a cash crop in the regional beer industry.

As adoption occurs, farmers tend to subdivide their farming system into commercial and subsistence sectors. The former are located on the best land, often with irrigation, and receive higher capital inputs. Although the subsistence sector is also important, it is relegated to poorer land and receives fewer inputs. It is possible that families experiencing land and/or labor shortage might eventually abandon the subsistence sector altogether. This relates to the frequency of off-farm employment and to greater reliance on purchased food.

Andean farmers have many reasons for preferring native over improved varieties, but culinary and agronomic ones stand out. Improved varieties are considered "watery", a negative attribute compared to the preferred "floury" quality of native varieties. The almost universal consensus among farmers interviewed about variety choice was that the new varieties were inferior eating potatoes, suited for soups, frying, or undiscriminating urban consumers. For subsistence farmers, whose diets remain predominately reliant on potatoes, keeping traditional varieties is a culinary necessity. No nutritional differences, other than water content, are noted between traditional and improved varieties. Improved varieties also do not remain viable as seed potatoes for more than a few seasons. Farmers observe that they degenerate and ultimately fail as seed. This can be overcome by purchasing new seed, but this is a costly requirement for subsistence farmers. Native seed potatoes are kept viable indefinitely by rotating seed between altitudinal zones, but the seed rotation system used for native seed are generally not applied to improved varieties.

Although farmers have reasons for maintaining traditional varieties, they have other reasons for adopting modern ones: their greater productivity and their resistance to the two greatest risks, frost and late blight *(Phytophthora infestans)*. Traditional varieties are generally recognized to be more susceptable to these risks than improved varieties. This challenges the notion that older types are more "adapted" than new ones.

Genetic erosion is undoubtedly occurring because farmers are changing their farming system. What is not known is the rate of genetic erosion relative to this change. Potato agriculture in commercially developed valleys such as the Mantaro and Cajamarca valleys has rates of replacement above 60%, but away from these areas the rate appears much lower, perhaps below 25%. There seem to be no areas in the higher altitudes, away from market centers or where subsistence production continues, where farmers completely abandon native varieties for improved ones or for other crops.

Genetic change is as inevitable and essential to subsistence agriculture as it is as to commercial agriculture. Subsistence farmers consciously select among clones, eliminating unwanted ones and emphasizing others. This selection is evidenced by the distribution of varieties within fields, by the cosmopolitan distribution of certain varieties over very large regions, by the use of certain types for gifts and exchange, by their cultivation for seed, and by the commercial status of a few native varieties. The contemporary pattern of genetic change continues patterns that predate improved varieties

and development programs, but with these new elements, the rate of change has accelerated, justifying the concept of erosion.

Besides the introduction of new varieties and other innovations for potato agriculture over the last thirty years, another major change is the rise of commercial potato production on the coast. Coastal production involves larger farms than are typical in the highlands, relies on purchased inputs, is commercially oriented, and employs improved seed produced under certification. The average yield on the coast is 17.85 metric tons per hectare while in the highlands it is only 5.86 metric tons (Peru 1975:2). Coastal potatoes are grown largely for the Lima market, only a short distance from the major center of coastal production in the Cañete Valley.

The successful development of potato agriculture on the coast is a potentially limiting factor in the rate of agricultural change in the highlands. Farmers in the central highlands compete in some of the same markets as coastal farmers, and the economies of scale, more optimal agroclimatic conditions, and proximity to markets of coastal producers give them great advantages over the highlands. This increases the risk to highland farmers of investing in new agricultural technology.

Besides competition with coastal producers, small farmers in the highlands face two opportunity costs in adopting improved potato varieties. First is the cost of diverting resources to change potato agriculture. Capital to purchase new seed can be invested in other crops, in livestock, or in other household production such as artisan activities. The Andean household economy is extremely diversified and variable, and given the uncompetitiveness of potato agriculture at higher altitudes, it is likely that available capital would not be diverted there. Second is the cost in time needed to make the adoption decision, learn about new varieties, locate and purchase seed. A significant portion of household income in all sectors of the Andes comes from off-farm employment, and these activities make more difficult an already complex agricultural calendar for farmers involved in a vertical economy. The time involved in the adoption decision may be an additional cost too great to bear. The result of these two opportunity costs is the decision to keep growing traditional varieties.

Although the data is insufficent to predict future trends of potato germplasm conservation and loss, we can identify factors that both promote and constrain genetic change. It is promoted by continued high population growth, increasing demand from urban markets, improvement in transportation and communication, changing consumption patterns, and active and well organized national and international crop improvement programs. It is constrained by population loss from some areas, consumer preferences, and the marginality of certain areas because of high risk, land fragmentation, and isolation from markets. The balance of these factors has resulted in the uneven pattern of change described above.

It is reasonable to assume that the balance will not be altered rapidly, for instance by technological breakthroughs that make remote highland regions more active in commercial potato markets or by a return to native varieties. It is possible that development programs could overcome these constraints to adoption and accelerate genetic erosion of potatoes in the highlands. The likelihood of this is diminished, however, by several features of the agricultural development policy of Peru. These include a serious financial crisis in the government, an emphasis on development in the tropical lowlands of eastern Peru rather than in the highlands, and a steady migration away from the highlands, especially from the small farm sector. These features are compounded by political pressures for cheap urban food from the coast as well as from abroad (Ferroni 1980), by ethnic differences between regions, and by failure of reforms to improve productivity in the highlands (Caballero 1981). These features indicate that the distribution of agricultural change and the adoption of improved potato varieties will not change in the near future.

Southeast Asian Rice Agriculture.—Rice *(Oryza sativa)* cultivation in Southeast Asia is a useful comparison to potato cultivation in the Andes. Unlike the Andes, where virtually all of the research on genetic diversity has been done in one country (Peru), Southeast Asia has similar research in several countries. Case study material from Thailand, Indonesia, and the Philippines demonstrates their similarity to Peru in terms of agricultural, economic and cultural diversity. Although the true center of domestication for rice is in northern India and southern China, the area of rice diversity extends into Southeast Asia (Chang 1976). Rice has been the object of intensive breeding and development programs for several decades, and modern varieties have diffused widely throughout the region. These include HYVs from the International Rice Research Institute (IRRI) and earlier releases of national breeding programs. Rice agriculture and the diffusion of new varieties have been examined in detail (Grist 1965; Hanks 1975; Barker 1978; Herdt and Capule 1983). Southeast Asia is characterized by different agroclimatic zones associated with hill and mountain environments. As in the Andes, the diffusion of improved varieties of rice corresponds to this zonation.

Reliable aggregate data on improved variety diffusion is more readily available for Asia than for the Andes. Economic and agronomic research on rice provides a good overall picture of the presence or absence of modern rice varieties (Herdt and Capule 1983), but little research has been done on the retention of diverse stocks that might accompany some adoption. Most research on adoption has concentrated in areas with relatively high acceptance of new varieties. These areas tend to be optimal for rice production and not "traditional" by virtue of their cultural diversity, ecological diversity or agroecological marginality (Huke and Duncan 1969). An exception to this pattern of research is work done in the Chang Mai Valley of northern Thailand (Rerkasem and Rerkasem 1984).

While lacking the varietal distinctiveness of a sexually propagated potato clone, rice is self-pollinating and thus typified by distinctive varietal populations (Grist 1965). Asian rice is subdivided into three major subspecies *(indica, japonica* or *sinica,* and *javanica)* which are geographically distinct and differ according to their grain. Each of these species is subdivided into numerous varieties whose grand total may be as high as 120,000 (Swaminathan 1984). In Indonesia, an estimated 8,000 varieties, primarily *javanica,* are found (Bernsten *et al.* 1982), while in the Philippines and Thailand, 1,500 and 3,000 varieties respectively are found with *indica* varieties predominating (IRRI 1978).

As in Andean agriculture, the genetic diversity of rice is recognized culturally in Asia. This recognition occurs at both the general level in the distinction between the major subspecies over large regions and at the specific local level. This specific cultural recognition is evident in naming and folk taxonomic systems, in planting patterns, in culinary aspects and in rituals. Among the Ifugao, H. Conklin (personal communication) has elicited a folk classification of five levels for rice. In 1973, this system was applied to the classification of 78 varieties grown by the Ifugao. Among the Iban of Sarawak, Sutlive (1978), found that each family maintained a variety of strains which were named according to such criteria as place or origin, bouquet, and culinary property. Sutlive describes the Iban practices of planting varieties in separate portions of the field and locating a special ritual segment in the middle of a field where a special variety is planted. At harvest time, the Iban gather each variety separately. Long and short grain rices are stored separately, and varieties that have not produced well that year are gathered and stored separately (V. Sutlive, personal communication). In Thailand, Rerkasem and Rerkasem (1984) note that farmers have a good appreciation for the concepts of "variety" and "selection". Farmers in Chang Mai identified 42 varieties from 55 grain samples according to color, shape and size of the grain and panicle branching and size (Rerkasem and Rerkasem 1984:304-305). All of this indicates the high level of cultural awareness and care for the genetic diversity of rice that is generalized across a wide area.

Rice cultivation in insular Southeast Asia is customarily divided into two major subsystems: upland (dry) rice and lowland (irrigated) rice. Variations exist (Padoch 1983), and irrigated areas may add a dry season rice crop which is not irrigated (Grist 1965). Upland rice is directly seeded and intentionally grown under aerobic conditions (McIntosh *et al.* 1984). Upland rice covered the majority of rice acreage in Asia, accounting for 61%, while irrigated rice covers 33% and deep water rice 6% (Huke 1982). In Southeast Asia, however, upland rice accounts for only a small perdentage of the total area—in Indonesia 11% and in the Philippines 14% in 1977 (IRRI 1984). Upland rice is associated with lower inputs (labor, capital) and also with lower productivity, averaging less than 1ton/ha compared to 4ton/ha for irrigated rice (IRRI 1984; Barker 1978). Sedentary agriculture accounts for most upland rice, although it is also found in swidden agriculture. In each it is primarily associated with subsistence production in which rice is supplemented by other crops (IRRI 1984).

Upland and lowland rice varieties belong to the same species but are distinguished along a continuum of morphoecological groups. Lowland rice exceeds upland in the number of varieties and genetic diversity. This would appear to contradict the image of a homogeneous agroecosystem for irrigated rice. Upland rice is distinguished by diversity relating to drought tolerance (IRRI 1984). It is associated with cropping systems that are generally more diverse and "traditional" than lowland ones. Chang (1976) suggests that the smaller diversity among upland types indicates that they are not ancestral to lowland types.

Lowland rice in Asia is famous for intensity and productivity. Geertz (1963) describes this as a product of "involution," an evolutionary process whereby increased labor is absorbed by fine-tuning the complex paddy system to achieve increased productivity. Although involution may be disputed (Collier 1981), the evolution of irrigated rice in the Philippines and Indonesia has resulted in high population densities, land fragmentation, and much higher yields than found in the upland systems. Densities associated with upland swidden systems generally fall below 50 persons per square kilometer while irrigated systems support 500 or more (Geertz 1963; Gourou 1966). On Java, which is dominated by wet rice, 63% of the land under cultivation is in farms of less than 0.5 hectares, and the average farm size is 0.64 ha (Birowo and Hansen 1981:5). On the Outer Islands of Indonesia, where irrigated rice is secondary to upland systems, the figures are the opposite, with 70% of the farms larger than 0.5 ha. On Kalimantan, where swidden is common, the average farm size is 2.71 ha.

For dry rice production in Indonesia, Gourou (1966) reports production of 812 kg. per hectare, while the Philippines wet rice employing traditional methods yields an average 2427 kg/ha (Conklin 1980). The disparity is even greater when modern varieties and other subsidies are applied to wet rice, and yields of over 4000 kg/ha are achieved (Barker 1978). Another distinctive characteristic of irrigated rice is the dominance of rice as a cultigen and subsistence item. Although rice is culturally significant among the Hanunoo, it accounts for less than 20% of their diet. Irrigated rice, on the other hand, is associated with monocropping and with heavy reliance on rice as the staple (Gourou 1966). Although the relative hegemony of rice decreased in Java during the Colonial period, it has continued in other parts of Southeast Asia (Geertz 1963).

A final distinction between the two systems is that irrigated rice is associated with an elaborate and stratified land tenure system in contrast to the simple and egalitarian system of tenure of swidden. The tenure system of paddy reflects the density of the population and the complexities of the water control system. Differential access is determined by ownership and a variety of rental and share tenancy arrangements (Utami and Ihalauw 1978; Conklin 1980). These create disparities between different sized farms and between landowners and the landless. Although the distribution of paddy is relatively egalitarian

among those who own land in Java, over 40 percent of the population is landless or near landless (Birowo and Hansen 1981).

The rate of change is greatest in lowland systems, that is in those that originally had the greatest diversity. As expected from this difference, the rate of genetic erosion in lowland rice greatly exceeds that of upland rice. Special agronomic characteristics, such as deep water or pathogens limit adoption, and cultural characteristics, such as preference for glutinous rice, may also be limiting. Considerable research has been devoted to the question of farm size as a limiting condition, concluding that it is not a significant factor (Herdt and Capule 1983).

Lowland rice systems have proved to be among the most dynamic small farm economies in the world, measured by the adoption rate of HYVs and other inputs. Herdt and Capule (1983:5) report that between 1966 and 1981, the overall percentage of Asian rice area planted in modern varieties rose from 1.4% to 39.5%. In countries where lowland rice predominates this rate is much higher. In the Philippines, for instance, by 1981 77.4% of the total rice area was planted in modern varieties. Although the Philippines represents an unusually high national average, numerous areas within other countries have achieved similar rates (Herdt and Capule 1983; Bernsten *et al.* 1982; Chang 1984).

The extent of genetic erosion in lowland rice in the Philippines is unusual in Asia, although the potential for replacement exists elsewhere. Indonesia, for instance, had 60% of all its rice land in modern varieties by 1981 (Herdt and Capule 1983). There is, however, some reason to speculate that HYV replacement may be limited in some lowland or irrigated rice systems. Prabowo and Sajogyo (1981) compare two villages on Java with widely differing adoption rates. In the East Java village, virtually complete adoption was reported, while in the West Java village, adoption rose and then sharply declined to 17% following an outbreak of gall midge. Poor water control and lack of fertilizer have limited adoption elsewhere (Herdt and Capule 1983).

Thailand is a striking contrast to the rapid diffusion of HYVs in the Philippines and Indonesia. By 1980, less than 10% of the Thai rice area was planted in HYVs, even though lowland rice predominates (Herdt and Capule 1983). Several reasons have been suggested for this low rate. Thailand is famous for high quality and glutinous rices, especially for export, and HYVs are not regarded as competitive in quality. Glutinous rice is preferred as a staple, and although glutinous modern varieties are available, they are interior to native varieties (Rerkasem and Rerkasem 1984). Fukui (1975) notes that native varieties performed as well as HYVs, given limited fertilizer use and underdeveloped water control. He observed that Thai farmers adopted HYVs not as a replacement to indigenous varieties but as a second crop because of their short growing period and non-photosensitivity. Rerkasam and Rerkasem (1984) describe the ethnobiological basis of non-adoption in the Chang Mai Valley. Their case study reveals that HYVs are grown on 5% of the land in the valley. Traditional varieties are grown on 20%, and two selected local varieties cover 55% of the land. These authors note that numerous criteria are used to evaluate rice varieties and that the farming system has many specific agroclimatic niches for which specific varieties are selected. It is unlikely that any single variety can meet all of these criteria or perform well across all niches.

Data from other lowland rice growing areas in the Asian center of diversity is insufficient to judge whether we should regard Thailand as typical of resistance to the diffusion of HYVs on the Asian mainland. Farmer (1979) notes that the new varieties have limited use during the wet season in India, Sri Lanka and Bangladesh, although they are widely used during the dry season, because of their non-sensitivity to photoperiod and their short duration. Regional wet season problems, such as deep water are important in limiting diffusion, as are problem soils in some areas (Farmer 1979). On Indonesia's outer islands, the percentage under improved varieties declines in higher elevations and areas under tidal influence and with toxic soils (Bernsten *et al.* 1982).

In South Kalimantan, for instance, modern varieties account for only 30% of the area in wet seasons and 20% in dry seasons (Bernsten *et al* 1982:21).

Besides these agronomic issues that limit diffusion of HYV rice, economic issues can also be identified. The first economic issue to be studied in relation to adoption was scale of farming, and this proved to be a non-issue. Small farmers were repeatedly found to be adopters, and in some areas, they actually exceeded larger farmers in adopting HYVs (Herdt and Capule 1983). In lowland rice, more marginal zones (as measured by degree of water control, access to major markets, etc.) are less likely to adopt than more optimal areas. This difference would appear to explain the relatively low adoption of modern varieties in Orissa (25%) compared to other Indian States, Andhra Pradesh (63%), West Bengal (41%) (Herdt and Capule 1983).

The most significant economic issue dividing high and low adoption is the difference between lowland and upland rice. Unlike lowland rice, the upland version is characterized by continued use of traditional varieties. Nepal with 75% of its rice in upland systems only achieved a 25% adoption by 1980 (IRRI 1984; Herdt and Capule 1983). In India, states with the highest proportion of upland rice show generally lower adoption of modern varieties. In Madhya Pradesh, with 40% of the rice area in upland, less than 2% is planted to modern varieties (IRRI 1984; Herdt and Capule 1983). Two limiting factors for adoption of improved rice varieties in upland farming systems have been observed: the dependence of improved varieties on water and the requirement for technical knowledge of and heavy investments in fertilizers (Kunstadter *et al.* 1978). These two characteristics make the new varieties inherently unacceptable to poor upland farmers.

Besides the inappropriate nature of HYVs for upland systems, the particular characteristics of those systems must be understood in relation to technological adoption. As noted above, these systems have always been less intensive than lowland ones. They are associated with subsistence rather than market product, marginal agroecological zones, and culturally distinct groups. Upland agriculture is often located in mountainous regions where slopes, soils and, socioeconomic conditions impede the construction of irrigation and terrace systems for paddy rice. Moreover, upland rice is usually only one component, often secondary, in diverse farming systems. The investment to learn about and acquire new varieties thus represents an opportunity cost to the other components.

DISCUSSION

Similarities and differences in the pattern of adoption of improved crop varieties are evident in comparing these two centers of genetic diversity. In both the Andes and Southeast Asia, the pattern of adoption is associated with a dichotomy between farming systems and with an upland/lowland dichotomy. In both, the genetic diversity of the major crop is retained in more marginal agroecological zones where farming systems are highly mixed and oriented toward subsistence. In the Andes, these zones are associated with higher altitudes where the environmental limits to the crop are more proximate. In Asia, these zones are also associated with hill agriculture and higher altitudes. In each area, advantages of the improved varieties are more apparent in the more intensive lowland system, where they are adopted. These systems are characterized by commercial production, greater control over the various inputs of agriculture, and better access to markets. The size of a particular farm does not seem to deter adoption in these areas. In some Asian rice systems, as in Andean potato systems, the adoption of new varieties is accomplished because they can fill special niches, such as in dry season rice farming.

In upland farming systems in both regions the advantages of improved varieties are outweighed or made irrelevant by other features of the system. In the Andes, high altutude potato farmers adopt new technology, but they do not replace older varieties with

improved types. The costs of seed, poor market access and culinary preference are the major limiting factors. In Asia, improved varieties of rice are not suited for specific lowland environments, such as deep water farming, and they are generally not acceptable to upland farmers who lack water control and other intensive management techniques. In each area, the availability of other critical inputs, such as fertilizers, influence the adoption of crop varieties.

The similarities between potatoes and rice in respect to genetic erosion may be summarized by reference to the importance of agroclimatic zone, cultural preference, and farming system differences regarding control over inputs, markets access, farm diversity, and subsistence orientation. Genetic change in each is more rapid in areas where intensive and commercial agriculture existed prior to improved varieties. In marginal areas, adoption is slower and is often accomplished by fitting improved varieties into a diverse farming system, rather than by simple replacement of the older seed stock.

While potato and rice farming systems share several attributes that indicate a general pattern of genetic erosion, it is also important to note that they seem to differ in the extent of change. Although it is impossible to generalize across all of Asia or the Andean region, rice in some Asian countries has experienced far greater change than potatoes anywhere in the Andes. This may be because new potato varieties do not outperform older ones to the same extent as new rice varieties. No Andean country in the original hearth of potato domestication has seen a majority potato fields planted in improved varieties, while several Asian countires have passed this mark in rice. These differences show clearly in a comparison between Peru and the Philippines. Unfortunately, regional and national data on potato agriculture in Peru is not comparable to information about Philippine rice, but the limited data available suggests that genetic erosion there is perhaps less than half the Philippine adoption rate of 77%. Although a majority of Peruvian farms in regions close to urban areas have converted to modern varieties, this rate drops sharply with distance from cities. With the exception of the upland/lowland rice dichotomy, the Philippine case shows a high overall adoption rate, regardless of location in relation to urban markets.

Several differences between the two crops and farming systems may be cited in relation to the more dynamic performance of rice in the Philippines. A primary one has to do with the national markets of each crop. Entering the 1960s, the Philippines experienced a serious deficit in rice production for its national consumption, and demand for rice has remained high. Peru, on the other hand, has only faced potato shortages in time of severe crop failure, and it has imported potatoes only in emergencies. The urban population of Peru consumes fewer potatoes than the rural one, and this keeps the potato market relatively weak. The strong national market in the Philippines seems to have reduced the disadvantages experienced by more isolated farms that are important in Peru. Transportation costs are relatively less for rice than for potatoes, and there is no comparable need to market the crop as a fresh vegetable.

Besides these market differences, two other factors may be cited to account for the lower rate of genetic erosion in Peru: a) different emphasis given to crop development in the two countries and b) fundamental agronomic differences between the crops. The high overall adoption of improved varieties in the Philippines followed large scale promotion campaigns which made credit, fertilizers, information, and improved varieties available. Faced with a serious deficit in rice production in the late 1960s, the Philippines undertook a successful agricultural extension program, the Masagana 99 program. This has been heralded as responsible for the dramatic changes in Philippine rice agriculture (Merrick 1981), typified by the adoption of HYVs. The success in improving Philippine rice production to self sufficiency through technological innovation is closely correlated with institution building in national research and extension programs.

Peru, on the other hand, has a generally poor record in improving agricultural productivity and a mixed record in institution building and extension. For the last 40 years, Peru has conducted national agricultural research programs aimed at improving potato production, and these programs have turned out over 30 improved varieties since 1950. Yet from 1954 to 1972, potato yields actually fell by 25% (Eastman and Grieshop 1986). No single campaign comparable to the rice programs in the Philippines was ever mounted in Peru. Peru has always been self sufficient in potato production (Eastman and Grieshop 1985) and urban food habits emphasize rice and bread rather than potatoes (Ferroni 1981). Agricultural research and extension suffered major setbacks in the 1968 military revolution, that attacked rural underdevelopment through agrarian reform rather than technological change. The National Potato Program ceased to function in the early 1970s, and the national agricultural extension service was abolished shortly after the military revolution. The National Potato Program was reorganized with CIP's help in the early 1980s, and the national agricultural extension service has been refashioned.

The adoption rates of new crop varieties in Peru and the Philippines are also affected by differences between the two crops. Perhaps the most important difference relative to genetic erosion is the ability of new rice varieties to outperform older varieties. In contrast, the newer potato varieties have some advantages, such as precosity or disease resistance, but their overall performance is not as clearly advantageous as rice HYVs. As an asexually reproduced and vegetatively propagated crop, the potato is disadvantaged by very slow seed multiplication. The time that it takes to produce a sufficient amount of new seed material in tuber crops is roughly ten times what sexually reproduced cereal crops require. The promotion of improved varieties in rice is enhanced by the ease of seed duplication, and by less cumbersome seed production and distribution systems. An important result of these differences is the availability and acceptability of greater amounts of improved rice seed to all types of farmers.

Another difference between the two crops is the complexity of seed maintenance in potatoes. As discussed above, native Andean agriculture employs a system of seed rotation between ecological zones. New varieties can be incorporated into this system, but farmers who adopt usually depend on regular purchase of new seed because of the degeneration of new varieties. This is a major limiting factor on the rate of adoption. In rice, on the other hand, no such cumbersome seed rotation system is required, and improved varieties have been easily incorporated into the pre-existing seed system. A third difference between potatoes and rice concerns storage. Potatoes, of course, are a fresh vegetable with a very high water content. In storage, they lose weight at water evaporates. Weight loss may be controlled by refrigeration, but this is not an option available to small farmers. As noted above, small potato farmers in Peru are in direct competition with larger farmers in the highlands or on the coast. Larger farmers who can afford refrigerated storage can play the market to their advantage by waiting until the price is advantageous. Small farmers without this storage capacity cannot afford to delay selling their crop. The result is that traditional varieties are kept for consumption and household income is sought from other sources.

The patterns of adoption and diffusion of new varieties of rice and potatoes described above are derived from the agroecology of the different farming systems producing each crop, by the socio-economic characteristics of these systems, and by the nature of the particular crop. Although retention of diverse, native germplasm in each crop is still evident, the ongoing evolution of these farming systems may later this picture. Systemic pressures for the adoption of improved varieties are present in both regions. These include rapid population increase, changing expectations and consumption patterns, and the generation of new technology more appropriate to marginal farm areas. This paper suggests that it is now advantageous for the farmers in these mixed agroecosystems to retain their ancestral landraces. Genetic change in these marginal areas will take renewed

and increased research and promotion and will require change in the overall agricultural system to make other technological inputs available. Competition with the more optimal farm regions already changed by the green revolution will retard this.

The conclusion is that genetic erosion does not occur evenly and will probably not proceed as rapidly as once expected. Agricultural development in marginal areas is but one of many alternatives for developing countries, and there are few institutional or political pressures to invest heavily in this. On the other hand, the people in these areas are as eager as any to improve living standards. As the world looks to ways to protect its genetic resource base, the continued cultivation of traditional varieties in marginal areas may be seen as a positive rather than a negative feature. The challenge for development planners is to devise ways to improve living standards of poor farmers and to encourage continued cultivation of traditional varieties. Conservation cannot and should not be pursued at the expense of the well-being of farmers who today manage the Neolithic legacy.

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SECTORAL FALLOWING SYSTEMS IN THE CENTRAL ANDES

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ABSTRACT.—Sectoral fallowing systems from 51 communities (Fig. 1) in highland Peru and Bolivia have been examined, focusing on altitudinal and latitudinal variation in these systems. In these instances of communal management of cropping patterns and fallowing, a significant proportion of lands is left fallow. This practice has been studied for implications related to maintenance of soil quality and reduction of pathogen impacts. Recent changes in several systems are noted. Comparison with other Andean patterns of culture and social organization reveal similarities with the present study area.

INTRODUCTION

Many peasants in the Andean highlands practice a complex pattern of crop and pasture management called sectoral fallowing systems. These systems, found in a large number of communities[1] distributed over a wide territory, are of interest to ethnobiologists for several reasons. They show that human knowledge and use of plants and animals can lead to the coordination of behavior of many individuals. They also demonstrate the complex interaction of human populations and the environments in which they live. Since there are a large number of these systems, they are particularly well-suited to statistical analysis.

Anthropologists who specialize in the Andean region have been drawn to study these systems, not only because they are an important aspect of economic organization in the region, but also because they appear to exemplify important forces which shape peasant life there. Some writers select an adaptationist perspective, viewing human activity as constrained by the difficulties of extracting resources from fragile and unproductive mountain ecosystems. Others emphasize the cultural continuities between earlier periods and the present, and see in agriculture and pastoralism, as in other areas of activity, underlying Andean patterns of thought and belief. Still others stress the importance of political economy, of the ways in which peasants and pastoralists, as one class in a dependent, stratified society, cope with the pressures from elites, governments and markets.

This article examines these sectoral fallowing systems in detail. It describes the basic features of these systems and analyzes the variation among the systems. The evidence provides general support for the adaptationist view of these systems, but also indicates the significance of Andean cultural patterns. It does not directly address the political economy perspective, neither confirming or challenging it. The evidence hints at recent shifts in these systems associated with population increases and commercialization of agriculture, but the lack of quantitative information does not permit a detailed exploration of these important links.

SECTORAL FALLOWING SYSTEMS: DEFINITIONS

Early ethnographic accounts of rural Andean life comment on community-wide management of cycles of planting and fallowing (Bandelier 1910:83, 85), but it has only been recently that these systems of land use have been described in detail. The account of this system by Matos Mar (1964) is one of the earliest full ones. The community of Paracaos in the province of Canta, department of Lima in the central Peruvian highlands may be taken as typical (Table 1). The 163 households which make up this community have access to some irrigated plots, which are cultivated each year, and to natural pastures. In addition, they have 929 hectares of land which they cultivate in some years and let lie fallow in others. These lands are divided into ten sectors. Most households own plots in all of the sectors. These households plant potatoes in their plots in a given sector in one year. In the next year, those who wish to do so plant other Andean tubers (oca,

TABLE 1.—*Cultivation cycle of sectoral fallowing systems in Paracaos (Degregori and Golte 1973:44-45).*

Name of Sector	YEAR 1958	1959	1960	1961	1962
Huayatama	pasture	pasture	pasture	pasture	pasture
Cushurumachay	pasture	pasture	pasture	pasture	pasture
Shujuncha	min. tubers[1]	pasture	pasture	pasture	pasture
Chuyochacra	potatoes	min. tubers	pasture	pasture	pasture
Acopuquio	pasture	potatoes	min. tubers	pasture	pasture
Ayal and Ushtuna	pasture	pasture	potatoes	min. tubers	pasture
Tamburhuasi	pasture	pasture	pasture	potatoes	min. tubers
Liuli and Mitaysinsan	pasture	pasture	pasture	pasture	potatoes
Sinsanchacra	pasture	pasture	pasture	pasture	pasture
Milacancha	pasture	pasture	pasture	pasture	pasture

Name of Sector	1963	1964	1965	1966	1967
Huayatama	pasture	pasture	potatoes	min. tubers	pasture
Cushurumachay	pasture	pasture	pasture	potatoes	min. tubers
Shujuncha	pasture	pasture	pasture	pasture	potatoes
Chuyochacra	pasture	pasture	pasture	pasture	pasture
Acopuquio	pasture	pasture	pasture	pasture	pasture
Ayal and Ushtuna	pasture	pasture	pasture	pasture	pasture
Tamburhuasi	pasture	pasture	pasture	pasture	pasture
Liuli and Mitaysinsan	min. tubers	pasture	pasture	pasture	pasture
Sinsanchacra	potatoes	min. tubers	pasture	pasture	pasture
Milacancha	pasture	potatoes	min. tubers	pasture	pasture

[1]minor tubers are olluco, oca, mashua

Oxalis crenata; ollucu, *Ullucus tuberosa*; and mashua, *Tropaeolom tuberosum)* in the same plots, although others choose not to plant in this second year. All of them let these plots lie fallow for the next eight years. Although individuals in the community recognize the boundaries and ownership of the plots, households have exclusive rights to use their plots only during the years when they are planted. During the fallow years, the plots are used as pasture by all the members of the community. The plots in all of the sectors pass through the same sequence of uses. However, the timing is staggered, so that no two sectors begin the sequence in the same year. When a particular sector is about to be cultivated after eight years of fallow, the community authorities indicate the day when agricultural activities will begin; they also ratify the ownership of particular households, and can redistribute plots which are abandoned or left vacant by owners who died without leaving heirs.

Other sectoral fallowing systems vary in the details of the crops which are grown, the length of the cycle, and the nature of the communal control, but they share with Paracaos a number of imporant features. (1) Each of the sectoral fallowing systems is a land-use system which consists of a set of lands associated with a set of households; (2) The set of lands are divided into a number *n* of sectors. The lands which make up each sector are contiguous; (3) All households own plots in most or all sectors. Most or all households own plots in all sectors; (4) There is a sequence of *n* year-long uses for the lands. Some of these uses consist of the planting of a specific annual crop or small number of annual crops. The set of crops may differ among the successive cropping uses out of the total set of uses. The other uses are fallowing, combined with grazing. All the fallowing uses occur after all the planting uses; (5) All *n* sectors pass through the same sequence of *n* year-long uses. In any given year, one and only one sector will have each cropping use, and the number of sectors which are fallowed is equal to the number of fallowing uses in the *n* year cycle; (6) When a sector is used for planting, each household has access and usufruct rights to its plot or plots in that sector; (7) When a sector is used for fallowing and grazing, all households have access and grazing rights to the entire sector; and (8) This land use pattern is maintained and enforced by institutionalized means.

These systems have awakened the interest of a number of anthropologists and geographers. They seem large in their spatial, temporal and social scale. Many specialists, accustomed to thinking of peasants as competitive and uncooperative, are surprised by this coordination of activity. How widespread are sectoral fallowing systems? How may their presence and distribution be explained? Along what dimensions do the sectoral fallowing systems differ from one another? Are there any regularities in their patterning? As in other areas of human activity in the Andes, several lines of explanation may be presented. The sectoral fallowing systems could be seen as adaptations to mountain environments, as a historical product of the political economy, or as an expression of underlying cultural patterns. These explanations could be integrated in a variety of fashions.

METHODS

Literature reviews conducted independently by the two authors (Custred and Orlove 1974, Campbell and Godoy in press) showed several types of sources: some detailed ethnographic studies of single sectoral fallowing systems, brief references to sectoral fallowing systems throughout the Andes, and a few studies of changes within sectoral fallowing systems in particular regions. We also sent a questionnaire to 119 anthropologists and geographers who have conducted research in the Andes. We received 27 replies. There were some instances of incomplete or missing information in both published materials and completed questionnaires. Our sample of 51 communities (Appendix 1) is thus not random. We hope that it represents a fairly complete presentation of available research on communities with sectoral fallowing systems.

There are several basic variables which we consider in detail and which are summarized in Table 2; though simple, they have not been fully distinguished in earlier writings. The cropping and fallowing cycle in any given system is as many years long as there are sectors in the system, since each sector must pass through every year of the cycle and since no two sectors are at the same point in the cycle in any given year. Thus the number of sectors which are in fallow at any given time is the same as the number of years which each sector lies fallow. We will discuss in particular the following variables: the number of years which each sector lies fallow, which we call "number of fallow years" and the number of years in which each sector is cropped, which we call the "number of crop years". The sum of these two variables is the number of years in the cycle, identical to the number of sectors in the system; we refer to it as "number of sectors". The ratio of the number of fallow years to the number of sectors is called the "fallow ratio". Because of the great importance of tuber crops, particularly the potato, in Andean agriculture, we constructed several measures.

The "number of tuber years" refers to the number of years in the cycle in which tuber crops are grown exclusively or predominantly. This definition raises certain measurement problems. If we counted only years in which tuber crops were grown

TABLE 2.—Summary of variables utilized in this study.

Description	Value	Name
Basic features:		
Number of tuber years	T	TUBERYR
Number of other (non-tuber) crop years	O	OTHCRPYR
Number of fallow years	F	FALLYR
Number of crop years	T + O	CROPYR
Number of non-tuber years	O + F	NOTUBYR
Number of sectors	T + O + F	NUMSECTS
Percentage of time in fallow	F/(T + O + F)	FLRATIO
Percentage of time not in tuber cultivation	(O + F)/(T + O + F)	NOTBRAT
Tuber years as percentage of crop years	T/(T + O)	TUBINDX
Other features:		
Mean elevation of plots in sectoral fallowing system		ELVSFS
Community elevation		ELVCOM
Number of sectoral fallowing systems associated with a specific community		NUMSFS

exclusively, we would have to reject cases in which a few other crops were occasionally interspersed in a sector otherwise planted with tubers. However, we did not wish to include cases where tubers accounted only for a small proportion of the crops which were planted in a specific year. In practice, this problem turned out not to be difficult; most sources described the cropping patterns in sufficient detail to permit the coding of years in which both tubers and other crops were grown as tuber years or non-tuber crop years.

The sum of the number of fallow years and the number of years in which crops other than tubers were grown is called the "number of non-tuber years". The ratio of the number of non-tuber years to the number of sectors is called, somewhat awkwardly, the "non-tuber ratio"; the ratio of the number of tuber years to the number of crop years is called the "tuber index". The tuber index ranges from 0.25, in the cases in which tubers account for only one of the four cropping years, to 1.0, when only tubers are grown.

In addition we sought to establish the mean elevation of plots within the sectoral fallowing systems; for this purpose, we used the much more readily available average of the highest and the lowest elevations within the sectoral fallowing system. To establish the elevation of the community, we took the elevation of the central settlement, if there was one, or the average elevation of the permanent dwellings, if there was not. These variables are respectively called "system elevation" and "community elevation".

The examination of the data revealed three difficulties which might be termed the multiple-system problem, the zero-fallowing problem, and the multiple-community problem. They are all illustrated by the community of Irpa Chico, in the province of Ingavi, department of La Paz, Bolivia. A recent monograph about this community offers one of the best descriptions of land use systems anywhere in the Andean highlands (Carter and Mamani 1982). The inhabitants on this community distinguish between the *sayaña*, a plot held by an individual or a household which can be used for grazing, cultivation or house construction; each household may decide independently on the use to which it will put each *sayaña* plot. An *aynuqa*[2], by contrast, is a set of communally controlled lands managed as a sectoral fallowing system. Local community authorities retain considerable discretionary powers in reallocating plots within these *aynuqa* to poor families. With a total of 513 households and 3109 hectares of land in the *aynuqa*, the land-population ratio for these lands is close to that in Paracaos. The total number of plots within the *aynuqa* exceeds 11,000. In Irpa Chico, unlike Paracaos and many other communities, there are several sectoral fallowing systems or *aynuqa* rather than just one. Carter and Mamani suggest that there are eight *aynuqa* within the community. The multiple-system problem consists in the necessity of deciding whether to consider each of the *aynuqa* as a distinct system. If all the *aynuqa* had the same sequence of planting anf fallowing, they might easily be considered to be a single sectoral fallowing system in which the sectors were non-contiguous. For example, if eight different *aynuqa* all went through a sequence of one year of potatoes, one of barley and six of fallow, then they could be considered to be a single sectoral fallowing system in which the plots in each sector were dispersed among sub-sectors rather than being placed contiguously. However, there are at least six different sequences (op. cit. 87-88). Some of these resemble other sectoral fallowing systems; in one, for instance, three years of cultivation, first of potatoes, then of quinoa *(Chenopodium quinoa)* and finally of barley, are followed by three fallow years. However, in other systems there are no fallow years at all. In one *aynuqa*, two years of potatoes are followed by four of barley, after which potatoes are planted once again. This zero-fallowing problem presents a dilemma: if the cases of zero-fallowing are excluded, then an artificial distinction is imposed between *aynuqa* which the local population all calls by a single term and manages by the same rules; their inclusion, though, seems to contradict the basic character of the vast majority of sectoral fallowing systems—the alternation of planting and fallowing.

Finally, the multi-community problem is presented by cases in which members of more than one community own plots within a single sectoral fallowing system. In Irpa Chico, each of the *aynuqa* are associated with a specific sub-community, which Carter and Mamani term *zona* or zones. Although Irpa Chico as a community shows a high degree of endogamy, there is also a fair degre of endogamy within the zones. Most households own the majority of the plots in the *aynuqa* associated with their zone, although many of them own plots in other *aynuqa* as well. If Irpa Chico did not possess other strong communal organizations, each specific zone might be considered to be a distinct community. There are several other cases, all within Bolivia, where there is some ambiguity over the question of whether the term "community" should be assigned to a larger, more inclusive social unit, which might manage several sectoral fallowing systems, or to a smaller, more exclusive one with a single system (Albó 1972, Buechler 1980, Campbell and Godoy in press). Their absence in Peru might be attributed to the policy of the Peruvian government of granting official recognition to communities; to receive such a title, a community must indicate precisely its boundaries and its component households. Their presence in Bolivia may also reflect a stronger continuity with the multi-level Andean *ayllu* form of organization (Platt 1982a). Because we both had field experience in the specific regions for which this multi-community problem appeared (Orlove in the Lake Titicaca area, Godoy in the northern portion of the department of Potosí), we believed that we could reasonably assign the limits to communities. It is quite possible that this multi-community problem appears in other areas, but has not been adequately documented.

The more troubling zero-fallowing problem appears to be less frequent than the multiple-system problem. After some reflection, we decided to exclude the zero-fallowing cases, in part because they proved to be very difficult to detect; Irpa Chico is the single instance in the literature in which we could be certain that plots in zero-fallowing systems were managed sectorally, rather than at the level of the individual household, although other sources suggest that it may occur elsewhere (Painter 1981). There appear to be many such non-sectoral zero-fallowing cases of potato cultivation on unirrigated land and of intensive maize cultivation on irrigated land.

The second justication for excluding zero-fallowing cases was the sense of Carter and Mamani's informants that such systems were recent and atypical, to be explained as the unfortunate consequence of land fragmentation. It thus seemed appropriate to consider such cases as former sectoral fallowing systems which would no longer be classified as such. Even this hedge did not completely eliminate the difficulty, because there were some cases in which fallowing was neither clearly present nor clearly absent. The island of Amantaní in the Peruvian portion of Lake Titicaca, with a four-sector system, is one such ambiguous case. When a particular sector is first cultivated, it is called *papasuyu* or 'potato-sector', since potatoes are the exclusive crop. The next three years, known as *oqasuyu* or 'oca sector', *siwarasuyu* or 'barley sector' and *uywasuyu* or 'livestock sector', reflect the dominant crops of the second and third years and the grazing on fallow in the fourth. Other crops, principally quinoa and broad beans *(Vicia faba)*, are also grown in the *oqasuyu*. In recent years, some households have grown broad beans in the *uywasuyu*; in the 1980-81 agricultural year, one of the authors [Orlove] observed such plots dispersed among the fallow fields. Since they covered much less than half of the area of the sector, and since local peasants still referred to the sector as *uywasuyu* rather than *awasuyu* ['broad bean sector'], it seemed appropriate to consider it as fallow.

The multiple-system problem was addressed by keeping distinct records for each of the systems, if a community controlled more than one. The analysis focusses on communities, for two reasons: the research upon which this study is based was conducted at the level of communities rather than of sectoral fallowing systems, and there are some

communities in which sectoral fallowing systems have been identified but in which the precise number of such systems is unknown. Most communities had only one system, but some had several. Of the total of 51 communities for which some information was available, 13 lacked sufficient information to establish precisely the number of sectoral fallowing systems. For the 38 communities with sufficient data, there were 55 sectoral fallowing systems. If analysis were conducted on one system per community, the systems from multi-system communities might be underrepresented in the sample; however, if the analysis were based on the total sample of systems, then the systems from multi-system communities might be overrepresented. This problem was far less intractible, since the analysis could be conducted both ways, and in fact gave similar results. The difference between single-system and multiple-system communities are slight, and the systems in both types of communities are similar. These topics are discussed in greater detail in the next section.

RESULTS

Distribution.—As earlier reports have indicated, the sectoral fallowing systems are restricted to a band in the central Andes (Table 3). They are found in central and southern Peru and in western Bolivia, ranging from 10° 20′.S to 18° 50′ S. Of the 51 cases, the elevations of the plots in the sectoral fallowing systems are available for 31; these range from 2400 to 4200 meters above sea level. The range of the average elevations of plots in the different sectoral fallowing systems is smaller, from 3000 to 4100 m. The rainfall, reported for 15 of the 51 cases, varies from 300 to 1270 mm/yr; excluding the highest and lowest figures, which may be suspect, reduces the range to 500 to 1100 mm/yr.

At higher elevations, crop production is limited to small plots of potatoes, predominantly the bitter varieties which are processed by freeze-drying into a product called *chuño;* the natural and cultivated grasslands in these areas support livestock. Fallowing is more extensive and is not managed communally. The fields at lower elevations are often irrigated. They are usually cropped yearly, in a more intensive agricultural regime. Other crops such as maize are more common, though some potato cultivation is also found. Fallowing in these zones is also not managed on a sectoral basis. Aridity seems to limit sectoral fallowing systems in the south, where fields are more often irrigated or fallowed on a non-communal basis. The factors which restrict sectoral fallowing systems to the north are less clear. Heavier precipitation and warmer temperatures affect soils and vegetation, favoring the formation of sod, quite different from the bunch grasses which characterize the main zone of sectoral fallowing systems. The management of this sod is relatively little known; shorter fallowing periods might be needed, for instance, or the greater labor requirements to prepare fallow fields might not be as well suited to sectoral fallowing systems. The apparent absence of sectoral fallowing systems north of 10° S. may also be the result of underreporting. Fewer anthropologists and geographers have worked there. Much of the published research, particularly for the northern Peruvian highlands, provides little detailed information on the spatial and temporal patterning of cropping, fallowing and grazing, although some sources clearly document the existence of non-sectoral fallowing systems (Brush 1977). The absence of these systems in Ecuador appears to be less in doubt.

This distribution places the sectoral fallowing systems well within the limits of cultivation of the potato, the most important crop in these systems. However, it is not quite appropriate to equate the sectoral fallowing system with potato cultivation, since Andean peasants at latitutdes outside the range of sectoral fallowing systems in northern Peru (Brush 1977), Ecuador (Knapp 1984) and southern Bolivia (Rasnake 1982) grow potatoes on plots that are fallowed. Other crops that are grown in sectoral fallowing systems, such as barley, quinoa, and broad beans, are also more widely distributed than

TABLE 3.—*Some Dimenstions of Sectoral Fallowing Systems.*

	S Latitude[1]	W Longitude[1]	Mean Annual Rainfall (mm)	ELVCOM (m)	ELVSFS (m)	CROPYR	FALLYR	NUMSECTS	FLRATIO	TUBERYR	NOTUBYR	NOTBRAT	TUBINDX	NUMSFS
Number of cases with data	48	49	15	43	31	44	43	42	42	41	39	39	41	38
Number of cases without data	3	2	36	8	20	7	8	9	9	10	12	12	10	13
Mean	14.34	71.70	728	3537	3725	2.602	5.24	7.91	0.640	1.463	6.70	0.8032	0.591	1.447
Median	14.25	71.50	725	3675	3800	3.000	5.00	7.00	0.631	1.500	6.00	0.8182	0.500	1.000
Trimmed mean[2]	14.33	71.72	719	3554	3753	2.612	5.11	7.85	0.643	1.459	6.62	0.8086	0.588	1.353
Standard Deviation	2.03	2.90	242	331	281	0.946	2.57	2.76	0.139	0.479	2.55	0.0837	0.246	0.724
Maximum Value	18.70	77.00	1270	3950	4098	4.000	13.00	15.00	0.867	2.000	13.00	0.9167	1.000	4.000
Minimum Value	10.40	66.10	300	2700	3000	1.000	1.00	2.00	0.333	1.000	2.50	0.6000	0.250	1.000
Third Quartile	15.63	73.35	850	3800	3910	3.000	7.00	10.00	0.766	2.000	9.00	0.8571	0.733	2.000
First Quartile	13.33	69.80	562	3200	3583	2.000	3.00	6.00	0.571	1.000	5.00	0.7500	0.333	1.000

[1]Latitudes and longitudes are stated in degrees and hundredths of degrees.
[2]The trimmed consists of the mean of the population from which the 5% of cases with the highest values and the 5% with the lowest values have been eliminated

these systems. We wish to stress that fallowing is found in many parts of the Andes where sectoral fallowing systems are absent.

Other Features.—A number of specialists on the Andes have emphasized the importance of the potato in agriculture in that region (Brush *et al.* 1981). In particular, potatoes are associated with agriculture at high elevations; in lower areas, maize is a major crop. Special frost-tolerant bitter varieties of potatoes are adapted to elevations too high for other crops. We were therefore surprised to find a slight negative relation[3] between the tuber index and the elevation of the community; we had expected a strong positive relation. In other words, potatoes account for a slightly higher proportion of the crops in communities at lower elevations.

TUBINDX = 2.04 — 0.000413 ELVCOM ($n = 37$, $t^{35} = -3.81$, corrected $R^2 = 27.3\%$). No significant correlation was found between the tuber index and the elevation of the sectoral fallowing system. An examination of the cropping sequences in Appendix 1 suggests that the importance of grains, particularly barley and quinoa, at high elevations may account for this unexpected result; it should be remembered that communities manage sectoral fallowing systems jointly with other crop lands at higher and lower elevations, so that cropping patterns within sectoral fallowing systems may be influenced by the availability of lands outside them. We also note that tuber indices decrease with distance from the equator:

TUBINDX = 1.24 - 0.0461 LATD ($n = 40$, $t_{38} = -2.68$, corrected $R^2 = 13.7\%$). The Mann-Whitney U Test finds the difference between the median tuber indices for Peru and Bolivia, 0.6 and 0.417, respectively, to be significantly different from Zero at the 0.05 level.

Another aspect of the sectoral fallowing systems remains to be discussed: the case of multiple systems. Of the 51 cases, sufficient data were available for 38 of them to determine the number of sectoral fallowing systems. There was one system in 25 cases, two systems in 10 cases, three systems in 2 cases, and there was also one case of a four-system community. In most communities with multiple systems, each system is located at a specific elevation. Camino *et al.* offer one of the best descriptions of such a community, Cuyo-cuyo in the province of Sandia in the Peruvian department of Puno (1981). In the highest system, the *luki manda* from 3800 to 4100 meters above sea level, a sector is first planted with special frost-resistant varieties of potatoes, called *luki* in Quechua and *papa amarga* ('bitter potato') in Spanish, which are used for the preparation of freeze-dried potatoes or *chuño*; the sector then remains in fallow for five years. In the *uray manda* from 3200 to 3800 meters, in the first year potatoes are planted, in the second oca, olluco and mashua, in the third oca, ulluco and mashua, and in some cases a few rows of broad beans, and in the fourth broad beans and some barley; the four crop years are followed by two years of fallow. The *manda del anexo* from 3200 to 32400 meters is located at some distance from the other two systems; it also follows a sequence of one year of potatoes, one of oca and four of fallow, although a number of other crops, including olluco, mashua and broad beans, may be included in both years. There is an association between the elevation of the community and the number of sectoral fallowing systems. The equation is:

NUMSFS = 4.36 - 0.000815 ELVCOM ($n = 35$, $t_{33} = -2.30$, corrected $R^2 = 11.2\%$). A Mann-Whitney U Test does not find significant at an 0.05 level the difference of the median elevations for single-system and multiple-system communities, which are 3720 meters and 3500 meters, respectively. This relation reflects the fact, discussed later in more detail, that the sectoral fallowing systems tend to be located higher than the community of peasants who cultivate plots in them. Since most multiple systems consist of systems more or less stacked vertically, the lower communities have more room in which to place these systems. The principal exceptions to these vertical arrangements are several multiple-system communities in the Lake Titicaca basin which,

like Irpa Chico, utilize distinct systems in locations whose differences, such as soil fertility, do not depend on large variations in elevation, in addition, the relative flatness of the Lake Titicaca basin impedes the downward drainage of cold air and create significant differences in the risk of frost between flat areas and adjacent slopes which are less than 100 meters higher in elevation.

We examined the different systems within multiple-system communities, and did not find significant correlations between elevation and the other features of such systems. In addition, Wilcoxon Signed-ranks Tests showed no significant differences between dimensions (number of sectors, number of crop years, tuber index, etc.), systems in single-system communities and multiple-system communities. On the basis of these results, we conducted our statistical analysis by taking the mean values of the systems in the multiple-system communities. Since 65.8% of the communities for which data were available had only one system, we did not believe that we were reducing variability by using means.

Adaptationist Hypotheses: Claims.—The adaptationist claims for the presence and distribution of sectoral fallowing systems have been stated on several occasions. Such claims tend to rest on rather loose notions of adaptation rather than on more rigorous extensions of biological concepts. Adaptations are seen as means by which populations draw resources from their environments while maintaining long-term productivity and by which they avoid the hazards present in their environments. One adaptationist discussion of sectoral fallowing systems states that fallowing, in combination with deposition of dung by animals grazed on fallow sectors, "is essential to the regeneration of the land" (Guillet 1981:144). Another article claims that these systems permit the "maintenance of soil fertility and removal of crop pathogens" (Brush and Guillet 1985:26). It is argued that Andean soils tend to be low in nutrients, that cultivation removes nutrients from the soils, and that fallowing must be maintained to prevent degradation of the soils.

Fallowing and Soil Management.—These adaptationist claims address issues that are more complicated than they might appear to be. Agriculture can lead to decreasing nutrient levels in the soil, because crop plants directly consume nutrients and because tillage can increase microbial activity which can lower nutrient levels and impair the physical structure of soils. However, fallowing does not necessarily restore nutrients to the soil, although it may do so, if nitrogen-fixing plants are cultivated or occur naturally. Fallowing may also increase the organic content of the soil, if atmospheric carbon fixed in photosynthesis during fallow returns to the soil; however, crop plants derive the bulk of their carbon from carbon dioxide in the atmosphere rather than from the soil. Fallowing nonetheless appears to present several advantages in the case of Andean agriculture. First, by reducing the proportion of the time when the soil is stripped of its plant cover, fallowing protects soil against some of the dangers of nutrient leaching and erosion that accompany agriculture. Second, fallowing may allow some soil nutrients to be redistributed in a fashion that makes them more avilable to crop plants. The deep-rooted perennial grasses which grow during fallow years reach to lower levels of the soil than annual crop plants do; they may draw nutrients from these lower levels and, when reincorporated into the soil during the preparation of fallow fields for planting, increase the concentration of these nutrients in the upper layers of the soil. Third, there may be agronomic benefits from the increase in organic matter which occurs as plants grow during fallow and then are partially or fully reincorporated into the soil; in particular, the physical structure of the soil may improve, enhancing its water-holding potential and favoring root growth. Fourth, the use of fallow land for grazing may also improve the soil. There may be a net transfer of nutrients to the fallow fields as animals grazed

on both fallow fields and on permanent pasture (often located adjacent to and above the sectoral fallowing systems) deposit dung on the fields; it should be noted, however, that the dung is often not deposited evenly throughout the fields, so that grazing in some areas might still lead to a loss of nutrients. (It is also possible, though unlikely, that the presence of grazing animals damages soils because of compaction.) Finally, fallowing may enhance the moisture content of the soil; this is its principal advantage in other regions of the world. It is noteworthy that a textbook, *Soils and Soil Fertility*, places its discussion of fallowing in the chapter on water management, rather than in any of the chapters on nutrients (Thompson and Troeh 1978). It is quite possible, though, that the addition of fertilizer to soils may enhance soil fertility more than fallowing does.

The relation between elevation and soil fertility is complex and mediated by several variables. Temperature is the most important of these, since it affects the rate of plant growth, the rate at which microorganisms convert nutrients such as nitrogen, phosphorus and sulfur found in organic matter in the soil into forms available for plants, and the rate of oxidation, which breaks down organic matter, and may therefore reduce the quality of the physical structure. At low temperatures, below about 5°C, biological activity in the soil is reduced to very low levels or stops entirely, so that the organic content of the soil does not increase and its physical structure does not improve (Price 1981:235). Higher temperatures cause organic compounds to oxidize and break down; at temperatures above about 25°C, this loss overtakes the formation of organic matter, and soil fertility declines. The meximum rate of accumulation of organic matter occurs between these extremes.

These rates, however, are influenced by soil moisture. Biological activity proceeds more slowly in very dry soils, reducing the decompositon of organic matter and the conversion of nutrients into forms available to plants. However, excessive moisture also reduces the oxidative decompositon of organic matter and the release of nutrients into the soil; it may also lead to the leaching of nutrients which are already present. As with temperature, there is an ideal range of moisture, somewhere on the wet rather than dry extreme.

There is thus no universal direct relation between elevation and soil fertility. Price (1983:248) states "soils in the humid tropics . . . generally improve with elevation (up to a certain point)," and offers as an example the contrast between the heavily leached soils commonly found in tropical lowlands and the more fertile soils in adjacent highland zones at intermediate altitudes. This difference is due primarily to the lower temperatures at higher elevations. In the case of the relatively high altitude regions where sectoral fallowing systems are found, however, it seems that soil fertility is lower at higher elevations because of low temperatures which slow the growth of bunch grasses and which impede decomposition of organic matter. It thus seems that sectoral fallowing systems are generally beneficial to Andean soils, because they assure a certain length of fallow period; it might also be important for fallow periods to be longer at higher elevations.

Fallowing and Pest Management.—The relation between fallowing and the reduction of losses due to nematode attacks is also somewhat unclear. The round cyst nematodes (*Globodera rostochiensis* and *Globodera pallida*) attack potato roots, reducing the ability of the plant to absorb water and nutrients (Evans *et al.* 1975). Losses increase with greater nematode density; in some cases, virtually no potatoes can be harvested. At one stage in the nematode's life cycle, the female nematode produces eggs which she retains within her body. When the female dies, her cuticle becomes a dark hard layer, forming a cyst inside which the eggs can survive for as long as 20 years (Hooker 1981:96). Nonetheless, nematode populations increase most rapidly when their populations are large and when food supplies are abundant; limitation of their food supply may lead their

populations to decline *(op cit.*:95). They are favored by moist cool soil conditions. Hooker states that "(w)hen nematode densities are high, rotation with potatoes grown once in five years is necessary to assure profitable potato yields. Resistant potato cultivars in rotation with susceptible cultivars and nonhosts considerably reduce the required length of rotation." *(op cit.*:96). This source does not provide a definitive fallow period necessary to prevent damage, since Andean peasants might well be able to subsist on potato yields which Western experts would not consider "profitable," and because many native cultivars may be resistant.

The number of non-tuber years is a better measure of the effect of particular sectoral fallowing system on the nematodes than the number of fallow years, since the effect on nematodes of a year in which grain and legume crops are grown is more likely to resemble that of a fallow year than that of a tuber year. It seems likely that a non-sectoral form of fallowing, in which the fallowing cycles of individual plots are not coordinated, would be less effective than sectoral forms, since plots adjacent to an area where nematode-infested potatoes remain at risk for several years. It would be more common in non-sectoral systems to find the planting of potatoes in plots adjacent to areas where potatoes had been planted in the previous year; in such plot, there is a risk that nematodes remaining from the previous year would disperse from the adjacent area to attack the new potato crop.

Other Claims.—In addition, sectoral fallowing systems have been considered to be adaptations in other ways. By grouping together a large number of plots that will be cultivated in a given year, they reduce the total perimeter of these plots which border on fallow land, thus reducing the frequency with which domesticated animals enter the plots and damage crops. This feature reduces the amount of labor which a household must allocate to the supervision of its animals (Mayer 1979:71). Skar's study (1982) of the Peruvian community of Matapuquio, in the province of Andahuaylas, department of Apurimac, shows the importance of limiting livestock damage. When an animal breaks into a plot in a sector currently under cultivation, the owner of the animal pays compensation to the owner of the plot; if the same animal were to damage crops in a plot that had been planted in a sector which was currently in the fallow portion of the cycle, no such payment is required. As a member of the community stated, "If you sow the other fields [in fallow sectors] then the animals will just come and eat it and no one is responsible for it." (1982:82) In addition, herders take particular care to keep animals away from cultivated sectors.

This juxtaposition of plots also permits the more experienced and knowledgeable members of the community to have a large role in making decisions about the timing of agricultural activities (Custred and Orlove 1974). If the plots were scattered, it would be more difficult for all members of community to obtain their advice.

Adaptationist Hypotheses: Results.—These adaptationist claims are generally supported by the fact that sectoral fallowing systems are located at higher elevations than areas of continuous cropping, where the maintenance of soil fertility is more problematic, and that they are associated with the cultivation of tubers, the crops most sensitive to round cyst nematodes. A more detailed examination of these systems does not support these claims unequivocally, however. The adaptationist claims refer to related but distinct aspects of the system. The fertility argument generally supposted that a significant proportion of the land will lie fallow at any moment, and usually does not address the question of how many years any one sector must remain without being cultivated. The pathogen argument, by contrast, states that each sector must remain out of tuber cultivation for a certain number of years, and usually does not address the question of what proportion of the time the sectors will remain without tuber cultivation.

The typical features of sectoral fallowing systems offer some support of the general claims. The mean fallow ratio in the 42 cases for which information is available is 0.640, a figure which seems high enough to permit the maintenance of the nutrient levels and the physical structure of the soil. One recent source makes the adaptationist claim that in sectoral fallowing systems "more land is left fallow than is being cultivated at any one time." (Guillet 1981:143-144). A use of the Wilcoxon Signed-ranks Test permits a non-parametric test of the hypothesis that mean fallow ratios are greater than or equal to 0.5; parametric tests, which require fallow ratios to be distributed normally, are less appropriate. This hypothesis is supported at the $p < .005$ level. However, 9 out of 42 cases have fallow ratios of 0.5 or less. Although these cases are part of a population whose mean is significantly greater than 0.5, they do not conform to Guillet's claim. In the 39 cases for which information is available, the average number of non-tuber years is 6.70 and 35 or 89.7% have at least 4 non-tuber years, probably enough to reduce pathogen populations (Hooker 1981:96). The hypothesis that the mean number of non-tuber years is greater than 4 is supported by the Wilcoxon Signed-ranks Test at the $p < .001$ level; nonetheless, 4 out of 39 cases or 10.3% had fewer than 4 non-tuber years.

These claims would be difficult to test definitively, since levels of soil nutrients and pathogen populations vary continuously, without sharp breaks at particular fallow or number of non-tuber years. The data do seem, though, both to support the claims and to indicate the existence of cases which challenge them. A second difficulty in definitively testing these claims lies in the fact that the same level of nutrient increase might be reached by different fallow ratios in different communities; a similar problem exists for pathogen protection. This issue could be addressed by examining variation by elevation. Since the benefits of fallowing accrue more slowly with an increase of elevation, because the colder temperatures at higher elevations reduce plant growth and microorganism activity, the fertility claim would suggest that the fallow ratio would increase the elevation. These relations do not hold; there is no significant relation between this feature of sectoral fallowing systems and elevation, whether measured by elevation of community ($n = 37$) or, more accurately but with a smaller sample, by average elevation of plots in the sectoral fallowing systems ($n = 26$). One might expect that more southerly communities, where average temperatures are lower and frost are more common, to require higher fallow ratios in order to achieve levels of soil fertility similar to those at equivalent elevations further north. In fact, the fallow ratio decreases with distance from the equator:

FLRATIO = 0.979 - 0.0236 LATD ($n = 40$, $t_{38} = -2.12$, corrected $R_2 = 8.2\%$). Since Bolivia lies generally further south than Peru, this difference could also be associated with the nation in which communities are located. The Mann-Whitney U Test comes close to finding a significant difference between the median fallow ratios, 0.571 in Bolivia and 0.652 in Peru; it assigns a p-value of 0.052. The lower fallow ratios in Bolivia might in part reflect the dominance of the Lake Titicaca region, where some fallow ratios are low, in the sample of communities from that country; the overall relation of fallow ratio with latitude is more puzzling.

Similarly, it should be noted that the number of crop years is positively associated with latitude:

CROPYR = 0.006 + 0.0186 LATD ($n = 42$, $t_{40} = 2.80$, corrected $R^2 = 14.3\%$), and that the differences for the median values for Bolivia and Peru, 3.5 and 2.5 respectively, is significant at the level of $p < 0.005$. There is no significant relation between number of sectors and latitude or nation. Noting the following relations:

FLRATIO = (NUMSECTS - CROPYR)/NUMSECTS

CROPYR = NUMSECTS - (NUMSECTS*FLRATIO),

it is reasonable that one has a negative and the other a positive relation with latitude.

It seems even more difficult to construct reasonable hypotheses for the influence of elevation or latitude on the nematode-reducing effects of fallowing. We did note an association between the number of non-tuber years and the elevation of the sectoral fallowing systems:

NOTUBYR = -8.09 + 0.00394 ELVSFS ($n = 25$, $t_{23} = 2.05$, corrected $R_2 = 11.7\%$).

The relationof the number of non-tuber years and the elevation of the community was weaker. Contrarily, we found a strong relation between non-tuber ratio and community elevation, though not with system elevation:

NOTBRAT = 0.422 + 0.000109 ELVCOM ($n = 35$, $t_{33} = 2.63$, corrected $R_2 = 14.8\%$).

These relations generally indicate an increase in non-tuber years and ratios with elevation.

Agricultural Intensification and Sectoral Fallowing Systems.—This examination of the adaptationist hypotheses led to another set of considerations. The adaptationist position might have been more strongly supported in the past than it is at present. Because of the extensive changes in highland Andean agriculture, it seems possible that some sectoral fallowing systems have been altered in certain ways. In this view, these weaknesses in the adaptationist position could be explained by the presence of forces which have shifted sectoral fallowing systems from a greater to a lesser degree of adaptation. In other words, it might be possible to accept the adaptationist position even though there are some cases which do not support it; to do so, one would need to find some means to distinguish between the systems for which it would hold and those for which it would not. One promising argument could be called the intensificationist position, since the reduction of fallow ratios resembles what Boserup (1965) calls agricultural intensification, and parallels her classification of agricultural systems. Population pressure and commercialization of agriculture have been identified as causes which might lead to more intensive land use within agricultural systems in general and sectoral fallowing systems in particular, reducing fallow ratios and the number of fallow years. Three sources which diachronically examine sectoral fallowing systems support these arguments. Godoy (1984) describes sectoral fallowing systems in the northern portion of the department of Potosí, Bolivia, in which the total number of sectors has been maintained, but each sector is cultivated for an increasing number of years and left in fallow for a correspondingly shorter period. He attributes this decline in fallow ratios, as well as the intensification in lands not cultivated by sectoral fallowing systems, to population increase; he also suggests that taxation placed additional demands on the yields of a finite land base. Mayer's (1979) studies of the Mantaro Valley in the department of Junín show not only that fallow ratios declined, but also that strong pressures for commercialization led to a breakdown of the coordination of fallowing and planting by different households, so that sectoral fallowing systems no longer exist. The previously-mentioned study by Carter and Mamani offers a detailed description of changes in the sectoral fallowing systems or *aynuqa* in one Bolivian community. In some cases, the number of years in the cycle is retained, but households cultivate crops in years that were formerly allocated to fallowing, much as some residents of Amantaní do. To follow the earlier terminology, the number of sectors in such cases remains unchanged, but a fallow year is replaced by a crop year. In other instances, an entire sector is taken out of the cycle and converted into private household plots, known locally as *sayaña*, so that the number of crop years remains the same but the number of fallow years is reduced by one. In either case, the fallow ratio declines; in some instances, when the number of fallow years falls to zero, the lands are no longer considered to be sectoral fallowing systems. Anecdotal information also provides evidence for this decrease in fallow ratios: an entire sector may be removed from the system by appropriation by a landlord (Orlove 1976) or by other communities (Godoy 1984). Since the number of sectors is reduced, the number of years in the cycle must decline as well. It seems that the number of crop

years remains the same and the number of fallow years is reduced, lowering the fallow ratio.

These pressures are documented by the sources already mentioned but not by the set of 51 cases. Information is scarce on the population variables; for example, none of the 51 cases have information on population density at the community level, and population densities at the next higher administrative level, of the district in Peru and canton in Bolivia, would be a poor substitute. Few cases had information on such possible measures of commercialization as distance to markets or presence of metal roofs. Fallow ratios and the number of fallow years also are not significantly related to other, less reliable measures of commercialization, such as the distance to the nearest town or official recognition of community. The 51 cases do offer some indirect support. There is a significant positive relation between the number of sectors and fallow ratio in sectoral fallowing systems.

FLRATIO = 0.426 + 0.0271 NUMSECTS ($n = 42$, $t_{40} = 4.04$, corrected $R^2 = 27.2\%$). The fact that there are few systems with a high number of sectors and a low fallow ratio suggest the generality of the following sequence: a system might start with a large number of sectors and a high fallow ratio. Demographic and commercial pressure might lead to a decrease in the number of fallow years and a corresponding increase in the number of crop years, leading to a decline in fallow ratio. After a certain point, the entire number of sectors might be halved and the area of each sector doubled (Mayer 1979:67-70). This possibility, though, is quite speculative. It should be stressed that the previously-mentioned areas where fallowing has declined are subject to high rates of population increase, increased sale of crops, or both of these influences. Fallowing may well be maintained in communities where populations have remained relatively steady, due to lower fertility, to higher mortality, or to higher out-migration, and where commercialization of agriculture has been less, due to acceptance by peasants of low cash incomes or to the earning of income through migratory wage labor.

This uneven support of the adaptationist position and the difficulty of directly testing the intensificationist position still leaves open the possibility that demographic or economic pressures have altered some features of the sectoral fallowing systems. It would therefore be useful to examine features of these systems which are less prone to these pressures, to search for regularities in them, and then to try to explain them. There are two such features: the relative elevations of the communities and the systems, and the sequencing of crops.

It would seem fairly likely that the former would change less than other features such as fallow ratios. A community might expand the sectors somewhat, add crop years and delete fallow years, or might lose sectors to haciendas, but it is less likely that the community or the sectoral fallowing system would be relocated entirely. The latter feature also displays certain regularities, for reasons that are less immediately evident.

In the case of the former, two variables, system elevation and community elevation, were compared. These variables are closely related:

ELVSFS = 2117 + 0.464 ELVCOM ($n = 27$, $t_{25} = 3.27$, corrected $R^2 = 27.2\%$). The system elevation is generally greater than community elevation. The mean difference between the system elevation and the community elevation is 229 meters; the Mann-Whitney U Test supports the hypothesis that this mean is greater than zero at the $p < .01$ level. The mean difference between system elevation and community elevation is unequal to the difference between the mean system elevation and the mean community elevation because of the different sample sizes for the two variables. There are several reasons for locations of communities at lower elevations: the importance of proximity to irrigated lands lying below the sectoral fallowing systems, the importance of deep river valleys in shaping transportation networks, the need for access to water. In some cases, this location also reflects the sixteenth-century Spanish policy of forcibly

resettling Andean populations in nucleated settlements called *reducciones*, which were often at lower elevations than the fields.

The crop sequences also show strong regularities. Commercial crops such as onions, cabbage and carrots are absent; such crops tend to be grown in lower zones, where warmer temperatures and more abundant irrigation water encourage investment in such crops. Conversely, subsistence crops dominate. More specifically, root crops have a position of unique importance. In 7 of 41 cases, only tubers are cultivated. In the remaining 34 cases, potatoes are always the first crop grown. In 11 cases, information is insufficient to determine precise crop sequenes. In the cases where there are several years of exclusive or mixed tuber cultivation, these years occur before the others in the cycle. To test the hypothesis that this pattern did not occur by chance, one could take the number of tuber years and the number of non-tuber crop years for each community and assume that the communities selected the order of tuber and non-tuber years at random. There is a probability of less than .001 that all 34 communities would place all tuber years before all non-tuber crop years.

This sequence of crops reflects the great importance of potatoes in Andean diets. They grow well over a wide range of environmental conditions (Gade 1975). Potatoes offer high yields, whether measured by unit area or by labor (Painter 1981; Brush *et al.* 1981). they account for a large proportion of the calories and protein which local peasants consume (Orlove 1986). In addition, they store well. It could be argued along adaptationist lines that Andean cultivators plant potatoes in the beginning of the cycle, because the levels of nutrients in the soil are highest at that time. If the concern was solely to provide potato crops with high levels of nutrients, though, they might be planted after, rather than before, nitrogen-fixing crops, such as the native tarwi *(Lupinus mutabilis)* and the introduced broad bean.

A second adaptationist perspective suggests that tubers are planted before grains and legumes because the harvesting of tubers destroys the pattern of furrows in the field, while the harvest of the other crops does not (Orlove 1977a). The purported advantage of this arrangement is that the furrows facilitate drainage of water in the rainy season, thus reducing erosion and permitting the earlier return of wild grasses. The evidence for this argument is anecdotal, and in any case this effect would be unlikely to hold with equal significance in the wide variety of degrees of slope and of soil structure in the regions where sectoral fallowing systems are found.

Culturalist Hypotheses.—Where adaptationists would view the needs and preferences of peasants as unproblematic consequences of environmental, technological and demographic causes, culturalists would seek to explore the origins and ramifications of these needs and preferences. In the instance of the priority given to the potato, a culturalist argument holds some plausibility. Andean peasants, it could be said, plant potatoes first because it seems right to them to do so. In this view, potatoes have a value which transcends their agronomic or nutritional significance. This culturalist argument is supported by the importance of potato-planting ceremonies in some regions with sectoral fallowing systems (Barstow 1979). Although the ritual emphasis on maize in the Andes is widely acknowledged to be greater than on potatoes (Murra 1960), potatoes hold a greater importance than the other crops with which they alternate in the sectoral fallowing system. No other crop would be as appropriate for the rituals that in many cases form an integral part of the preparation of a sector after it has completed its fallow years.

There are other cases where Andean cultural patterns structure fallowing cycles. As previously mentioned, in Paracaos, potatoes are planted in the first crop year, known as *turno* (Degregori and Golte 1973); the second year, known as *lana* or *yana*, the minor tubers are grown, and a smaller proportion of the plots are planted. The term *yana* has

great significance in the Andes; it refers to the second, and lesser, or two elements which form a complementary duality (Platt 1978).

This correspondence might suggest that a certain number of sectors represents the cultural ideal; one might expect an even number of sectors, granted the importance of dualism in Andean culture. Although the link between cultural pattern and observable behavior need not be direct, this view nonetheless is not supported by the empirical evidence. The proportion of even values of the number of sectors, the number of crop years and the number of fallow years is not significantly different from the proportion of odd values, and in some areas where Andean traditions are held to be particularly strong, there are odd numbers of sectors; the Jukumani in northern Potosí, with a seven-sector system, are a case in point.

The culturalist perspective may account for particular details, but its greatest significance is its accounting for relative smoothness with which sectoral fallowing systems operate. This view resembles Bourdieu's discussion of culture as habitus, which he defines as "systems of durable, transposable dispositions" (1977:72; Ortner 1984). Three features of sectoral fallowing system which are common in the Andes are boundedness, universal participation and rotational turns. The existence of well-bounded communities, all of whose members participate in many activities, is reflected in a number of spheres: other communal patterns of resource management, such as pasture and irrigation water (Brush and Guillet 1985); the importance of assemblies in which all resident households and no outsiders have a vote (Alberti and Mayer 1974); collective work projects such as road and school construction, in which all resident households must contribute equally in labor and in materials (Brush and Guillet 1985); and the well-known ritual complex of the annual cycle of fiestas (Isbell 1978; Rasnake 1982). Many aspects of Andean organization show similar patterns of the division of a whole into equal parts, and the assignment of these parts on a rotational basis (Albó 1974). Common examples include the rotation of political and ritual offices on a yearly basis among the sections of a community (Rasnake 1982) and the rotation of political offices among villages in multi-community systems (Albó 1972). The management of irrigation water, in which water rights are assigned sequentially to a number of component households, is another example of this pattern of rotational turns (Mitchell 1976). This instance also reflects the boundedness and universal participation in the limitations on use of water by outsiders and in the ritualized annual cleaning of irrigation canals, in which all households participate. An unusual example of rotational turns may be found in bands of musicians who play panpipes at fiestas around Lake Titicaca; Buechler (1980:55) reports that these instruments "are always played in pairs with successive notes alternating between them." Two recent studies examine the division of space into a set of equal parts from this culturalist perspective (Wachtel 1982; Urton 1984). these instances render plausible the potential culturalist argument, that Andean peasants manage sectoral fallowing systems well because they have had experience with organizing themselves on such a basis in other areas of activity, and find this basis fair and reasonable.[4]

Political Economic Hypotheses.—Writers who emphasize political economy view these forms of organization somewhat differently. They argue that the tight internal governance and great emphasis on ritual in Andean communities are to be understood primarily as responses to pressures from external elites which seek to control peasant land and labor (Wolf 1982:145-149). In this sense, the sectoral fallowing system serves to mark large extensions of land as being equally under the protection of a set of households. In plots managed by households, it would be more difficult to protect them against encroachment from others. Although only 7 sources report the extent of sectoral fallowing systems, the average number of households, about 350, may well represent a balance between the advantages of small size (easy management) and large size

(protection against outsiders). Similarly, it could be argued that sectoral fallowing systems are well-suited to regions like the Andes, where seasonal migration encourages peasants to manage their plots in a highly predictable manner. In fact, the areas where sectoral fallowing systems are now found correspond loosely to the provinces from which in the colonial period *mita* workers were sent to Huancavelica and Potosí, though this spatial overlap is far from precise. Nonetheless, this emphasis on political economy is not directly supported by the case material. Many such systems are found in areas where there is relatively little pressure from outsiders on peasant lands and where there is relatively little outmigration. Though political economy in general has influenced Andean agriculture strongly (Orlove 1977b), sectoral fallowing systems do not seem to provide evidence for this view.

SUMMARY

There are certain regularities among sectoral fallowing systems. They are found within specific altitudinal and latitudinal limits. They may serve to maintain soil fertility, to reduce nematode damage to potato crops, and to facilitate a mixed agropastoral economy. Specific diachronic information from selected localities indicates that fallowing decreases with population increase and with commercialization; since such demographic and economic data are available for only a few communities, it was not possible to examine these relations by a statistical analysis of a large number of sectoral fallowing systems. The sectoral fallowing systems also correspond to other social and cultural patterns in the Andes; this link may help explain their relatively smooth functioning.

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NOTES

[1]In many parts of the Andes households can be unambiguously associated with a local set of households whose lands are continguous and whose members participate in institutionalized economic, political and religious activities. The term "community" is a common and convenient label for such groups. It is important to mention, though not to dwell upon, the major debates among Andean specialists over the meaning and appropriate use of this term: to what extent do communities reflect Andean cultural principles and patterns of social organization; to what extent are current community structures the product of interaction with imperial and national states; to what extent are patterns of community organization a spontaneous outgrowth of adaptation to environments; in what ways do communities interrelate with other patterns of local-level social organization (Guillet 1978; Guillet and Brush 1985; Murra 1984; Salomon 1982; Yambert 1980).

[2]The transcription of Quechua and Aymara terms is not fully standardized. We have adopted the spelling of the source which we cite most frequently. The same term also appears in published sources as *aynoca* and, less frequently, *aynoqa*.

[3]We do not offer exact p values for the acceptance or rejection of the relations, since a precise t-test is inappropriate. The variations are not normally distributed, and the necessary condition for equality of variance is not met. Nonetheless, we have included regressions for which t-ratios are greater than 2.0, since such levels seem to indicate some importance.

[4]We have deliberately avoided a discussion of the origins of sectoral fallowing systems, in part because they are so difficult to detect in both the archaeological record and historical sources. Their conformity with Andean patterns of organization suggests an Andean origin, although there are similar patterns of organization in Spain (Fernandez 1981, Espinoza 1981).

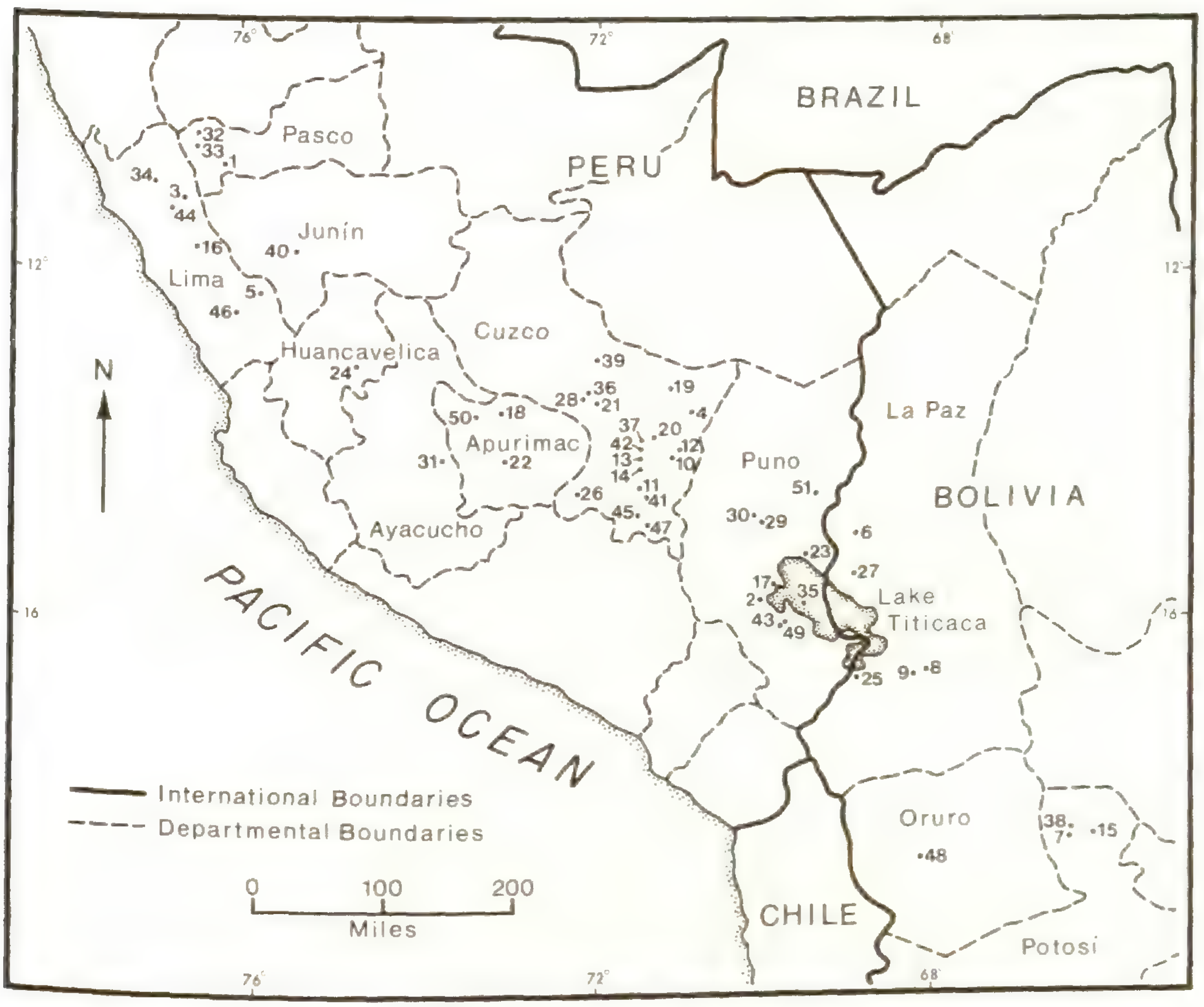

FIG. 1. Locations of communities studied. See text and appendix 1.

APPENDIX 1.—Communities with sectoral fallowing systems.

Name of Community	Department	Country	°S Latitude	°W Longitude	Elevation of Community (meters	Total No. of SFS per Community	Average Elevation of SFS (meters)
Paucartambo (Andrews 1963)	Pasco	Peru	10° 40′	76° 20′	3044	1	N/A
Paucarcolla (Bourricaud 1967)	Puno	Peru	15° 40′	70° 10′	3830	1	N/A
Santa Lucía de Pacaraos (Degregori & Golte 1973)	Lima	Peru	11° 15′	76° 40′	3200	1	3425
Marcapata (Yamamoto 1982)	Cuzco	Peru	13° 30′	70° 55′	3100	3	3300
Same							3850
Same							4100
Miraflores/ Laraos (Mayer and Fonseca 1979)	Lima	Peru	12° 20′	75° 50′	3000	1	3500
Kaata (Bastian 1978)	La Paz	Bolivia	15° 0′	69° 0′	3500	1	4100
Jukumani (Godoy 1982)	Potosí	Bolivia	18° 35′	66° 25′	N/A	1	3750
Jesús de Machaca (Vellard 1963)	La Paz	Bolivia	16° 35′	68° 5′	3800	1	3800
Irpa Chico (Carter & Mamani 1982)	La Paz	Bolivia	16° 40′	68° 20′	3800	2	N/A

No. of Years Fallow[1]	No. of Years Culti-vated	Total No. of Sectors	% of Years Fallow	Crops in 1st Year	Crops in 2nd Year	Crops in 3rd Year	Crops in 4th Year	Crops in 5th Year
4	1	5	80%	Potatoes	Fallow	Fallow	Fallow	Fallow
N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
8	2	10	80%	Potatoes	Oca Mashua Olluco	Fallow	Fallow	Fallow
3	2	5	60%	Potatoes	Oca Olluco Mashua	Fallow	Fallow	Fallow
3	2	5	60%	Potatoes	Oca Olluco Mashua	Fallow	Fallow	Fallow
1	1	2	50%	Bitter Potatoes	Fallow	Bitter Potatoes	Fallow	Bitter Potatoes
7	3	10	70%	Potatoes	Oca Mashua Olluco	Barley	Fallow	Fallow
5	3	8	63%	Potatoes	Oca	Barley	Fallow	Fallow
3	4	7	43%	Potatoes	Oca Olluco Mashua	Wheat	Barley	Fallow
3	4	7	43%	Potatoes	Quinoa Cañihua	Cañihua	Barley	Fallow
3	3	6	50%	Potatoes	Quinoa	Barley	Fallow	Fallow

[1]In some instances, some plats will be cultivated in the first fallow years. Note discussion in text.

APPENDIX 1. Communities with sectoral fallowing systems. (continued)

Name of Community	Depart-ment	Country	°S Lati-tude	°W Longi-tude	Elevation of Com-munity (meters)	Total No. of SFS per Com-munity	Average Elevation of SFS (meters)
							N/A
Santa Bárbara (Orlove & Custred 1974)	Cuzco	Peru	14° 10′	71° 10′	N/A	1	N/A
Quehue (Orlove & Custred 1974)	Cuzco	Peru	14°	71°	3675	2	N/A
							N/A
Pata Anza (Orlove & Custred)	Cuzco	Peru	14°	71°	N/A	1	N/A
Pampa Marca (Orlove & Custred)	Cuzco	Peru	14° 5′	71° 30′	3829	1	N/A
Mosocllacta (Orlove & Custred)	Cuzco	Peru	14° 10′	71° 30′	3815	1	N/A
Laymis (Harris 1982)	Potosí	Bolivia	18° 30′	66° 15′	N/A	1	3900
San Pedro de Casta (Gelles 85 Question.)	Lima	Peru	11° 45′	76° 35′	3168	1	3000
Rinconada (Chaquilla 85 Question.)	Puno	Peru	15° 45′	69° 55′	3820	2	4050
Same							3820

No. of Years Fallow[1]	No. of Years Culti-vated	Total No. of Sectors	% of Years Fallow	Crops in 1st Year	Crops in 2nd Year	Crops in 3rd Year	Crops in 4th Year	Crops in 5th Year
3	3	6	50%	Potatoes Ocas Mashua	Habas Arvejas Quinoa	Barley	Fallow	fallow
10	3	13	76%	Potatoes	Olluco Oca Habas	Barley Habas Quinoa Cañihua	Fallow	Fallow
8	1	9	88%	Potatoes	Fallow	Fallow	Fallow	Fallow
7	2	9	77%	Potatoes	Barley	Fallow	Fallow	Fallow
6	3	9	67%	Potatoes	Oca Olluco Habas	Habas Quinoa Cañihua	Fallow	Fallow
3	3	6	50%	Potatoes	Barley Olluco Quinoa	Barley Olluco Quinoa	Fallow	Fallow
4	3	7	57%	Potatoes	Barley	Habas	Fallow	Fallow
N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
2	2	4	50%	Potatoes	Arvejas Habas	Fallow	Fallow	Potatoes
3-5	3	8	63%	Potatoes	Potatoes	Oca Olluco	Fallow	Fallow
5	2	7	71%	Potatoes	Barley	Fallow	Fallow	Fallow

[1]In some instances, some plats will be cultivated in the first fallow years. Note discussion in text.

APPENDIX 1. Communities with sectoral fallowing systems. (continued)

Name of Community	Department	Country	°S Latitude	°W Longitude	Elevation of Community (meters)	Total No. of SFS per Community	Average Elevation of SFS (meters)
Matapuquio (Skar 1982)	Apurimac	Peru	13° 35′	73° 10′	3000	1	3750
Ch'eqec (Webster 85 Question.) (Pseudonym)	Cuzco	Peru	13° 20′	71° 10′	3300	3	3950
Same							3550
Same							2650
Usi (McCorkle 85 Question.) (Pseudonym)	Cuzco	Peru	13° 45′	75° 30′	3700	2	3750
Same							3750
Chinchero (Núñez del Prado 1949)	Cuzco	Peru	13° 25′	72° 0′	N/A	1	N/A
Caraybamba (Fujii/Tomoeda 1981)	Apurimac	Peru	14° 10′	73° 5′	3140	1	3750
Chanajari (Collins 85 Question.)	Puno	Peru	20°	30°	3740	1	N/A
Huallay Grande (Bradby 85 Question.)	Huancavelica	Peru	12° 55′	45°	3600	1	3900
Chiripa (Browman 85 Question.)	La Paz	Bolivia	16° 30′	69° 0′	3835	1	3910
Alccavitoria (Custred 1974)	Cuzco	Peru	14° 30′	72° 20′	N/A	1	3800

No. of Years Fallow[1]	No. of Years Culti-vated	Total No. of Sectors	% of Years Fallow	Crops in 1st Year	Crops in 2nd Year	Crops in 3rd Year	Crops in 4th Year	Crops in 5th Year
5-8	4	12	66%	Potatoes	Oca Olluco	Habas	Wheat	Fallow
9	1	10	90%	Potatoes	Fallow	Fallow	Fallow	Fallow
5	1	6	83%	Potatoes	Fallow	Fallow	Fallow	Fallow
2	1	3	66%	Maize	Fallow	Fallow	Maize	Fallow
3	2	5	60%	Potatoes	Barley Habas	Fallow	Fallow	Fallow
3	2	5	60%	Potatoes	Barley Habas	Fallow	Fallow	Fallow
4	3	7	57%	Potatoes	Olluco Ocu	Barley Habas	Fallow	Fallow
13	2	15	87%	Potatoes Olluco Ocas	Volunteer Potatoes	Fallow	Fallow	Fallow
7	4	11	64%	Potatoes	Ollucos Ocas	Habas	Barley	Fallow
6	1	7	86%	Potatoes	Fallow	Fallow	Fallow	Fallow
4	3	N/A	N/A	Potatoes	Barley Wheat Quinoa Cañihua	Habas Tarni	Fallow	Fallow
N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A

[1]In some instances, some plats will be cultivated in the first fallow years. Note discussion in text.

APPENDIX 1. Communities with sectoral fallowing systems. (continued)

Name of Community	Department	Country	°S Latitude	°W Longitude	Elevation of Community (meters)	Total No. of SFS per Community	Average Elevation of SFS (meters)
Ambaná (Aguirre 1980)	La Paz	Bolivia	15° 25′	69° 0′	3800	2	3700
Same							4500
Rumipata (Pseudonym) (Guillet 1979)	Cuzco	Peru	N/A	N/A	3100 +	1	3500
San Jose′ (Jacobsen 1982)	Puno	Peru	14° 50′	70° 5′	3800	1	N/A
Llaulli (Jacobsen 1982)	Puno	Peru	14° 50′	70° 5′	3800	1	N/A
Soras (Turpaud & Murrugarra 1966)	Ayacucho	Peru	14° 5′	73° 35′	3433	1	3400
Tangor (Mayer 1979)	Pasco	Peru	10° 25′	76° 25′	2700	1	N/A
Yacan (Fonseca 1972)	Pasco	Peru	10° 30′	76° 30′	3667	1	3780
Yancao (Galdo & Martínez 1966)	Lima	Peru	11° 0′	77° 0′	N/A	2	N/A
							N/A
Taquile (Matos Mar 1957)	Puno	Peru	15° 40′	69° 35′	3830	1	N/A

No. of Years Fallow[1]	No. of Years Culti-vated	Total No. of Sectors	% of Years Fallow	Crops in 1st Year	Crops in 2nd Year	Crops in 3rd Year	Crops in 4th Year	Crops in 5th Year
5	5	10	50%	Potatoes	Ocas	Habas Peas	Barley	Oats
10	3	13	77%	Potatoes	Ocas	Oats	Fallow	Fallow
4	3	7	57%	N/A	N/A	N/A	Fallow	Fallow
2	4	6	33%	Potatoes	Barley Oats Quinoa Ocas Habas	Repeat	Opt. Repeat	Fallow
5	N/A	6	83%	N/A	Fallow	Fallow	Fallow	Fallow
4	3	7	57%	Potatoes	Ocas Ollucos Mashua Habas	Barley	Opt. Repeat	Fallow
7	5	12	58%	Potatoes	Ocas	Trigo	N/A	N/A
5	3	8	63%	Potatoes	Oca Olluco Mashua Quinoa	Barley Habas	Fallow	Fallow
4	3	7	57%	Potatoes	Ollucos Ocas	Barley	Fallow	Fallow
2	1	3	66%	Bitter Potatoes	Fallow	Fallow	Bitter Potatoes	Fallow
N/A	N/A	6	N/A	N/A	N/A	N/A	N/A	N/A

[1]In some instances, some plats will be cultivated in the first fallow years. Note discussion in text.

APPENDIX 1. Communities with sectoral fallowing systems. (continued)

Name of Community	Depart-ment	Country	°S Lati-tude	°W Longi-tude	Elevation of Com-munity (meters)	Total No. of SFS per Com-munity	Average Elevation of SFS (meters)
Tukiwasi (Pseudonym) (Guillet 1974)	Cuzco	Peru	N/A	N/A	3100	1	3304
Moccoraise (Gade 1975)	Cuzco	Peru	13° 55′	71° 30′	3500	1	N/A
Macha (Platt 1982)	Potosí	Bolivia	18° 30′	66° 20′	N/A	1	3850
Yucay (Fiorante Molinie 1982)	Cuzco	Peru	13° 0′	72° 0′	2930	1	N/A
Mantaro Valley eastside (Mayer 1979)	Junín	Peru	11° 45′	73° 20′	N/A	2	4100
2nd Mayer							3750
Surimana (Orlove 1976)	Cuzco	Peru	14° 30′	71° 25′	3586	2	3700
							N/A
Pomacanchi (Gade 85 Question.)	Cuzco	Peru	14° 0′	71° 30′	3700	1	3825
Ichu (Bourricaud 1967)	Puno	Peru	16° 0′	69° 50′	3830	1	N/A
Huayapampa (Fuenzalida 1968)	Lima	Peru	11° 20′	76° 50′	3047	2	3275
Same							N/A

No. of Years Fallow[1]	No. of Years Culti-vated	Total No. of Sectors	% of Years Fallow	Crops in 1st Year	Crops in 2nd Year	Crops in 3rd Year	Crops in 4th Year	Crops in 5th Year
1	1	2	50%	Potatoes Ocas Quinoa Habas	Fallow	Repeat	Fallow	Repeat
7	4	11	64%	Potatoes	N/A	N/A	N/A	Fallow
7	4	11	64%	Potatoes	Oca	Quinoa	Barley	Fallow
N/A	N/A	9	N/A	Potatoes	N/A	N/A	N/A	N/A
10	2	12	83%	Potatoes	Barley Oats	Fallow	Fallow	Fallow
4	3	7	57%	Potatoes	Oca Ollucos	Wheat Barley	Opt Repeat	Fallow
5	3	8	63%	Potatoes	Barley	Habas	Fallow	Fallow
7	2	9	77%	Potatoes	Barley	Fallow	Fallow	Fallow
2	3	5	40%	Potatoes	Barley	Habas	Fallow	Fallow
2	3	5	40%	Potatoes	Barley Oats	Quinoa	Fallow	Fallow
3	3	6	50%	Potatoes	Olluco Oca	Wheat Barley	Fallow	Fallow
5	2	7	71%	Tuber	Tubers	Fallow	Fallow	Fallow

[1]In some instances, some plats will be cultivated in the first fallow years. Note discussion in text.

APPENDIX 1. Communities with sectoral fallowing systems. (continued)

Name of Community	Department	Country	°S Latitude	°W Longitude	Elevation of Community (meters)	Total No. of SFS per Community	Average Elevation of SFS (meters)
Huayhuahuasi (Orlove 1977)	Cuzco	Peru	14° 40′	71° 30′	3950	1	N/A
Huancaire (Soler 1964)	Lima	Peru	12° 25′	76° 10′	3200	1	N/A
Pampahuarca (Orlove 1977)	Cuzco	Peru	14° 50′	71° 25′	3900	1	4100
Escara (Prescott 1974)	Oruro	Bolivia	18° 40′	68° 10′	N/A	1	N/A
Acora (Tshopik 1946)	Puno	Peru	16° 0′	69° 50′	3861	2	N/A
							N/A
Uripa (Fonseca & Murrugarra 1966)	Apurimac	Peru	13° 35′	73° 25′	2903		3675
Cuyo Cuyo (Camino 1978)	Puno	Peru	14° 30′	69° 30′	3450	3	3950
							3500
							3300

No. of Years Fallow[1]	No. of Years Cultivated	Total No. of Sectors	% of Years Fallow	Crops in 1st Year	Crops in 2nd Year	Crops in 3rd Year	Crops in 4th Year	Crops in 5th Year
10	2	12	83%	Potatoes	Cañihua	Fallow	Fallow	Fallow
N/A	N/A	6	N/A	Potatoes	N/A	N/A	N/A	N/A
9	2	11	82%	Potatoes	Quinoa Cañihua	Fallow	Fallow	Fallow
N/A	3	N/A	N/A	N/A	N/A	N/A	Fallow	Fallow
3	2	5	60%	Potatoes	Quinoa Barley	Fallow	Fallow	Fallow
10	2	12	83%	Potatoes	Cañihua	Fallow	Fallow	Fallow
N/A	N/A	N/A	N/A	Potatoes	Fallow	Fallow	Fallow	Fallow
5	1	6	83%	Bitter Potatoes	Fallow	Fallow	Fallow	Fallow
2	4	6	33%	Potatoes Mashua	Oca Olluco Mashua Habas	Habas Barley	Fallow	
4	2	6	66%	Potatoes	Oca Malle Lisas Mashua Yacon Rachacha	Fallow	Fallow	Fallow

[1]In some instances, some plats will be cultivated in the first fallow years. Note discussion in text.

Additional cases of sectoral fallowing systems were recently reported by Dr. Xavier Albo for the department of Cochabamba, Bolivia, at locations generally above 3000 meters in elevation and between 17° S. and 18° S., 65° 30′ W. and 67° W. Local Quechua names for these systems include *ayanuga*, *ayta*, *muyu* and *manta*.

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OPTIMAL FORAGING: SIMULATION STUDIES OF DIET CHOICE IN A STOCHASTIC ENVIRONMENT

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ABSTRACT.—Foraging models are a growing part of the ethnobiological study of hunter-gatherers. Highly simplified for analytic reasons, the early "classical" versions of these models excluded consideration of several relevant factors. Among them is the possibility of maladaptive food shortages due to the effects of stochastic environmental fluctuations on food capture rates. To remedy this deficiency, the present simulation study develops a stochastic, risk-minimizing analog to the widely used but deterministic and rate-maximizing diet choice model. The results indicate that optimal efficiency-maximizing and risk-minimizing choices are quite similar. However, even with the optimal risk-minimizing choice, the forager in a stochastic environment will experience large variations in food capture rate from one time interval to the next. This and other studies using and developing foraging theory hold the promise of a micro-ecological theory of hunter-gatherer economies, past and present.

INTRODUCTION

Human and evolutionary ecology have met in the study of hunter-gatherers. The result is a modest but promising and growing place in the ethnobiological niche, one where anthropologists and biologists are applying common theories to the study of like topics. Research on the foraging behavior of human and non-human organisms is a prominent part of this effort. This work, based in optimal foraging theory, typically uses highly simplified models to examine food or habitat choice, time allocation, the use of space, or group formation and structure.

The theory and methodology of foraging models and applications in anthropology recently have been reviewed elsewhere (Pulliam 1981; Winterhalder and Smith 1981; Smith 1983; Winterhalder 1986a). A lucid and comprehensive review of the biological literature on this subject can be found in Stephens and Krebs (1986). Applications of interest to ethnobotanists are quite diverse, and include hominid paleoecology (Hill 1982; Kurland and Beckerman 1985), archaeology (Bettinger and Baumhoff 1982; O'Connell, Jones and Simms 1982; Webster and Webster 1984) and human ecology (Hames and Vickers 1982; Beckerman 1983; O'Connell and Hawkes 1984; Hill *et al.* 1985; Pate 1985; Sih and Milton 1985; and Smith 1985). Thomas' (1985) attention to this body of ideas, in his paper on the 'state-of-the-art' in hunter-gatherer studies in archaeology, highlights its growing importance.

Here I would like to focus on an extension of a key component of foraging theory, the diet breadth model. Although its analytical utility is well established, the diet breadth model excludes a long list of variables which might affect resource selection. In fact, on first encounter, the set of missing considerations appears more impressive than the caloric benefits and costs, search and pursuit times, and prey densities which are the actual stuff of the model. It includes the effects on foraging behavior of: (i) non-foraging activities; (ii) environmental stochasticity and risk; (iii) group interactions, including sharing; (iv) resource depletion; (v) incomplete information (uncertainty); and, (vi) non-

energy utilities (e.g., nutrients and non-food materials). While this tally of omissions might seem a staggering debility, it also can be viewed as an exciting queue of questions, hard on the heels of a promising analytic beginning.

This paper is one of a series which attempts to refine understanding of the diet breadth model by examining the role of factors excluded in the original formulation. Two previous publications (Winterhalder 1983, 1986a) have looked at the interaction of foraging and other behaviors by modelling the effects of opportunity costs on diet choice. The present paper posits a risk-minimizing (rather than the usual rate-maximizing) goal for the forager, and thus begins to incorporate stochasticity of environmental variables into the diet choice model. A subsequent paper (Winterhalder 1986b) examines the effects of food sharing on foraging choices. This series is an attempt to develop more fully a micro-ecological understanding of human diet selection.

BACKGROUND AND DEFINITIONS

Current analysis of foraging behavior is based largely on deterministic models, those assuming that an average or expected value for each of the relevant parameters suitably characterizes it. The emphasis is on efficiency, defined as maximizing a net rate of intake while foraging. Such models are relatively easy to devise and test, and they may sometimes approximate the results of their more complex stochastic analogs (Poole 1979; Stephens and Charnov 1982). Nonetheless, what is convenient for the analyst may be alien to the forager. Environmental variability may be the more apposite factor in selection for behavior (Winterhalder 1980). Random variation may be common in nature and broadly significant in the analysis of evolutionary phenomena like foraging (Real 1980; Real *et al.* 1982). For the organism, effectively avoiding risk due to this variability may be more important than efficiently adapting to its circumstances, viewed normatively.

In this paper I report the results of a study in which parameters in the diet breadth model (MacArthur and Pianka 1966; Schoener 1971) are treated as stochastic variables. This model predicts which among a set of potential prey an optimal forager will elect to pursue. It has been used extensively in studies by ecologists and anthropologists (reviews in Pyke *et al.* 1977; Krebs *et al.* 1983; Smith 1983; Stephens and Krebs 1986; Winterhalder 1986a). According to the model, the parameters central to foraging decisions are encounter rate (based on search rate and prey density) and pursuit and handling time for each species. The forager has energy costs associated with search and pursuit; benefits are determined by the net energy value of the prey captured. This analysis will introduce a stochastic element into each of these two basic components of the model, encounter rate and pursuit time. Although conceivably there are other ways to approach the question of diet choice and risk, this particular entry to the problem has the advantage that it begins with simple modifications in the structure of a familiar deterministic model. I have adopted a simulation methodology for finding the stochastic solutions to the diet breadth model (equation 3) because a direct analytic analysis proved unwieldy.

Risk commonly refers to the potential for a significant decrement to the fitness of an organism associated with a particular situation, choice or activity (see below). Environmental stochasticity can but need not induce risk; it is only one among the factors (e.g. predation or exposure) which might be considered more generally as hazardous. Further, this paper considers risk (from stochastic variation or unpredictability) rather than uncertainty (from incomplete information). Lack of appropriate information may contribute to unpredictability in an organism's environment, but the analytic and evolutionary implications of this situation are different from the case of stochasticity (Stephens and Krebs 1986). In order to keep these questions clearly separated, here I assume that the organism "knows" the environment to the extent possible—it has complete information on the probability distributions for the stochastic foraging parameters.

Because unsuccessful foragers can exist for some period on endogenous reserves, with only gradually increasing costs to adaptive performance, risk assessments must include a temporal dimension. Short-falls may not matter, or may matter to varying degrees, depending on their severity and duration, and on the nutritional state of the organism at their onset. The model presented here assumes that there is a critical foraging interval which will end to the detriment of an organism experiencing a relatively low or negative energy balance. Foraging benefits will be measured by the net acquisition rate for energy (see Smith 1979).

The formulation of a stochastic foraging model requires three steps: Step 1, specify how fitness (through a currency or utility) will be defined for a particular problem (e.g. avoiding starvation, maximizing net acquisition rate); Step 2, specify the relationship between that aspect of fitness and the structure of the food reward (e.g. mean, variance); and Step 3, specify the relationship between the structure of the food reward and behavior (e.g. patch residence time, diet breadth). For clarity, these steps will be identified in the analysis which follows.

In the existing economic and ecological literature risk has two meanings, best kept separate. As above, it sometimes refers to fitness by identifying the possibility that some subsistence decisions in an unpredictable environment may threaten survival and reproduction. In this use risk is probability of loss. This is a Step 1 consideration and in deference to colloquial use will be designated risk. In other cases risk has been used to refer to choice among possibilities with differing variances (e.g. "risk-prone" or "risk-averse," Caraco 1981), meaning to choose or avoid reward variability. To also use risk in this context invites ambiguity. Hence, for this second case I will speak of variance-prone or variance-averse (cf. Caraco 1983). This is a Step 2 consideration. By these definitions, all organisms should be risk-averse, but one interesting prediction of models described below is that to achieve this goal they may seek, or try to avoid, variability, depending on circumstances. With reference to foraging, risk-sensitive means generally that the variance of food capture rates associated with resource decisions affects fitness.

RISK-SENSITIVE BEHAVIOR

Selection is expected to generate adaptive strategies which compensate for environmental variation, if it affects fitness. Experimental study has revealed that two species of juncos (Caraco *et al.* 1980; Caraco 1981) have consistent preferences in simple tests with reward variability. Given a choice between a variable and fixed reward with equivalent means, birds feeding at a rate *above* their requirements choose the constant reward (the are variance-averse), while those feeding *below* intake requirements choose the variable reward (they are variance-prone). Further experiments with white-crowned sparrows, and a choice between two degrees of variability at a fixed mean, led to similar results (Caraco 1983): consistency of choice in a pattern predictable from the overall energy budget.

To help explain these observations, Stephens (1981) assumed that the organisms were trying to minimize their probability of starvation, and he derived rules for risk-sensitive foraging which predicted Caraco's results. For the same expected (average) intake, a forager in positive energy balance will choose the minimum variance; the forager in negative energy balance will choose maximum variance. This has been termed the "extreme variance rule" by Stephens and Charnov (1982).

Stephens and Charnov (1982) argued that the conditions for the extreme variance rule are unlikely in nature. Rarely would an organism face a choice between fixed and probabilistic food awards *at the same mean*. These authors derived a more realistic and general solution for the risk-sensitivity problem which analyzes sets of mean/variance

combinations. In their "Z-score model" encounter rates are given by a poisson distribution, but they assume a limiting distribution for net acquisition rate that is normal.

The fitness goal of the risk-sensitive organism in this model is to avoid falling short of some threshold, a starvation net acquisition rate, given as R_{min}. This is a Step 1 procedure. A simple graphical model (Fig. 1) depicts the optimal choice from among complex mean/standard deviation (s.d.) sets. Formally, the chance of starvation increases with the function

$$(R_{min} - \mu_t)/\sigma_t \quad (1)$$

where R_{min} is the minimum net acquisition rate tolerable, and μ_t is the average and σ_t the s.d. of the food reward distribution. It is evident that increasing the tolerable minimum, or decreasing the average reward, will increase the organism's risk. The role of the variance is more complicated: when the numerator is negative, increasing variance increases risk; when positive, increasing variance reduces risk.

If Z′ is set equal to (1), then minimizing Z′ will offer the forager the greatest chance of avoiding its crisis threshold. By rearranging terms the relationship can be depicted in the form of the familiar quadratic (or slope-intercept) equation.

$$\mu_t = R_{min} - \sigma_t Z' \quad (2)$$

Given a set of mean/s.d. pairs, the line of greatest slope (-Z′), drawn from R_{min} and intersecting a point in the set, represents the organism's best chance of avoiding starvation (Fig. 1). In effect, this procedure allows the Z-score of a standard normal distribution to accomplish Step 2 of a risk-sensitive foraging analysis. It also identifies the optimal mean/s.d. pair, and by working backwards from that combination it is possible to identify the appropriate behavioral choice (Step 3).

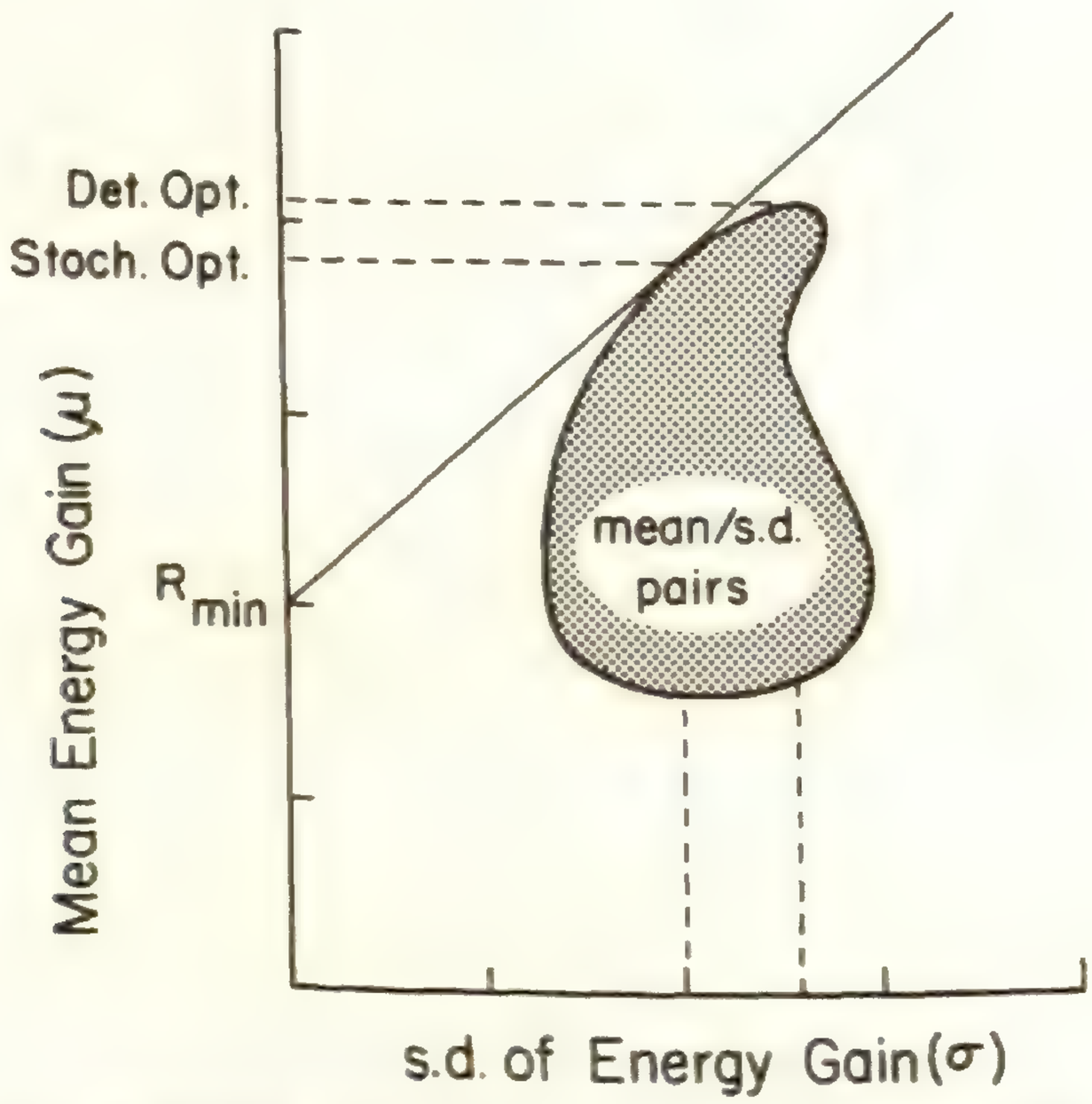

FIG. 1.—Stephens and Charnov (1982) risk-sensitive foraging model. The steepest line extending from R_{min} and intersecting a point in the feasible set of mean/s.d. pairs gives the optimal risk-minimizing solution, with a mean and standard deviation (E/T) as shown (stoch. opt.). Also shown are the mean and s.d. of the rate-maximizing solution (det. opt.).

Stephens and Charnov (1982) show that the extreme variance rule is a special case of their formulation, in which the feasible set is given by horizontal lines (fixed μ_t, but differing σ_ts), either above or below the organism's threshold R_{min}.

Stephens and Charnov apply their general result to the analysis of optimal patch residence time, given stochastic variation in the search time required to find a new patch. This is the stochastic analog to the marginal value theorem (Charnov 1976). The result is an oblong mean/s.d. set which leads to these inferences: (i) the optimal patch residence time in the stochastic or risk-sensitive case is near to that for the deterministic, rate-maximizing solution; and, (ii) if the organism is in positive energy balance (net acquisition rate $>$ R_{min}) then the optimal residence time is somewhat longer for the stochastic case (the organism is variance-averse), whereas for a net acquisition rate $<$ R_{min} it is somewhat shorter (and the organism is variance-prone).

The convergence between the efficiency-maximizing and risk-minimizing choices is an important result of this analysis. But, the unusual shape of the mean/s.d. set associated with this particular foraging model suggested to Stephens and Charnov (1982:259) that "it may be misleading to try to find general relationships between mean and variance . . . this relationship may depend critically on the nature of the foraging problem at hand".

The present analysis will use simulation methods to generate stochastic variation in the parameters of the diet breadth model in order to test this proposition.

METHODS[1]

The objective is to calculate the mean/s.d. pair associated with each possible diet breadth in a resource choice situation (Step 3). The set of these pairs then can be used to identify the optimal, risk-sensitive diet choice (Step 2), using the Z-score model. I will assume that the fitness-related goal of the organism (Step 1) is to avoid falling below some minimum level of food intake, measured as energy net acquisition rate.

The methodology and results will be described in three steps, first with stochasticity in encounter rates only, second with stochasticity in pursuit times only, and lastly, with stochasticity in both search and pursuit. Some species may face unpredictability in encountering potential prey, but little stochastic variation in the time required to pursue and handle an item. Others may experience regular encounters, but face unpredictability in pursuit times. Yet others, perhaps most foraging species, will face some degree of stochasticity in both parameters affecting diet breadth decisions.

The Diet Breadth Model.—The diet breadth (db) model specifies that a prey item should be added to the diet only if its net return relative to pursuit and handling costs (e_x/t_x) is greater than the average efficiency for the diet containing all items of higher rank. This statement can be represented mathematically (see Schoener 1974):

$$e_x/t_x > \frac{\Sigma p_i e_i - C_s T_x}{\Sigma p_i t_i + T_s} = E/T \qquad (3)$$

with variables defined as follows:

e_x net energy (kcal) of prey item x (its energy value less the energy required to pursue and handle it).

t_x time (min) required to pursue and handle prey type x.

p_i the relative frequency of prey type i, as encountered, calculated for those prey in the diet at a particular diet breadth.

e_i net energy value (kcal) of prey type i (see e_x).

C_s the cost (kcal/min) of searching for prey.

T_s the average search time (min) required to encounter an acceptable prey item (one included in the diet at a particular diet breadth).

t_i the time (min) required to pursue and handle prey type i (see t_x).

Σ sum from prey type i = 1 to i = (x - 1), with prey ranked from most to least valuable by e_i/t_i.

E/T overall foraging efficiency at a particular diet breadth.

The optimal diet includes all ranked prey items down to but not including the type x for which the inequality in equation 3 reverses.

Search time (T_s) is derived for each diet breadth by the formula

$$T_s = \frac{1}{(V_s 2r_s) \Sigma d_i} \quad (4)$$

where

d_i prey density (#/km^2) for prey type i.

V_s search velocity (km/min).

r_s search radius (km).

Σ sum for those prey types in the diet at that diet breadth.

This equation assumes that the search time to locate an acceptable prey type is inversely related to the velocity and radius of the search, and to the aggregate density of the prey types that will be pursued when encountered.

Stochastic Search.—The model assumes that prey are encountered randomly. In the deterministic version this means that the d_i for each type is the average density. In the stochastic analog, any finite interval of searching will cover a fixed area which is a sample of the randomly located resources. Prey densities actually encountered in that interval will be distributed as a stochastic poisson variate (d^*_i) with $\lambda_i = d_i$, for i = 1 to n potential prey types.

The program determines the stochastic density variate (d^*_i) for each prey type by generating a random number (range 0.001 to 0.999) which is matched to intervals of the cumulative poisson distribution for the associated λ_i. Each cumulative interval is associated with an integer value for d^*_i. This procedure generates 100 unit area samples in which the density of each species is a stochastic poisson variate, d^*_i. Foraging efficiencies are calculated for each diet breadth in each interval, followed by the mean and the s.d. of the foraging efficiency at each diet breadth for all one hundred intervals. The value of the input parameters and the size of the unit area searched have been set arbitrarily (see Table 1). If no prey are encountered, then the foraging efficiency for that interval is set at the negative of the search cost ($-C_s$).

In effect, this procedure envisions an organism which must allow for foraging efficiency variation due to stochastic encounter rates among sequential foraging periods.

TABLE 1.—*Prey and predator parameters used in the deterministic and stochastic analyses.*

Name	Energy Value (Kcal)	Pursuit Time (Min)	Pursuit Cost (Kcal/min)	Density (#/km^2)
Prey Parameters:				
APREY	200.0	4.0	3.0	0.4
BPREY	150.0	30.0	3.0	0.8
CPREY	80.0	10.0	2.0	1.0
DPREY	40.0	1.0	3.0	4.0
EPREY	20.0	1.0	3.0	8.0
FPREY	100.0	40.0	2.0	10.0
Forager Parameters:				
Velocity: 8 km/hr				
Search Radius: 0.2 km				
Search Cost: 2.0 Kcal/min				

Its risk-sensitive foraging goal is to avoid a critical net acquisition rate in each unit area searched. The procedure assumes that encounter rates are not correlated from one interval to the next, nor are densities among prey types within an interval correlated. This may generate greater degrees of randomness than a forager actually would experience, given the possibility of co-variation among contiguous samples of habitat, or local interactions among prey species in the use of food or space. However, if prey are clumped relative to the scale of the forager's search area, then it could underestimate unpredictability.

Stochastic Pursuit.—In the deterministic model, encountered prey within a particular diet breadth are always pursued; there are no "partial preferences." This assumption is retained in the stochastic case. Consistent with the assumption of unpredictability, the organism is assumed to know the probability distribution for pursuit of an encountered species, but is assumed not able to estimate pursuit time in any given encounter. Pursuit times are given as a stochastic variate t^*_i, with a normal distribution, $= t_i$. The s.d., $\sigma = 0.25\mu$, is scaled positively with the mean by a constant coefficient of variation.

Random numbers (range 0.00003 to 0.99997) are matched to intervals of the cumulative normal distribution, generated by taking Z-scores in half-unit increments from $-4 \lessdot Z \lessdot +4$. The Z-score at the mid-point of the randomly selected interval is then converted to the pursuit time variate by the standard formula ($t^*_i = \mu_i + Z\sigma_i$). In effect, this forager faces normally distributed stochastic variation in the time required to pursue, capture and consume a given prey item.

As indicated above, the program calculates foraging efficiency (E/T) at the end of a foraging interval based on a unit area of searching. Within that interval the forager may encounter and pursue zero, one or more of a particular prey type. If it pursues more than one, the pursuit time variate used to calculate the efficiency for that foraging interval is the average of the variates (t^*_i) for each of the encounters. This recognizes that the stochastic pursuit time variance for prey which are commonly encountered and quickly pursued will to some degree "even out" over the predator's critical foraging interval.

Stochastic Search and Pursuit.—This program joins the previous two procedures. For each of the 100 calculated intervals, the density of each prey type *and* its pursuit time(s) are treated as stochastic variates.

RESULTS

Table 1 shows the data set used in all simulations. Table 2 shows the prey ranking, the computed foraging efficiency at each diet breadth, and the optimal, deterministic solution. This data set was constructed with values which might characterize a small mammalian or avian predator, and then adjusted so that it gave (a) an optimum at an intermediate diet breadth and (b) a range of foraging efficiencies.

TABLE 2.—*Optimal foraging analysis for the deterministic case.*

Prey Type	Rank (e_x/t_x, in Kcal/min)	Foraging Efficiency (E/T, in Kcal/min)
APREY	47.0	1.85
DPREY	37.0	7.63
EPREY	17.0	9.94
— — — — — — — — — — — Boundary of Optimal Diet — — — — — — — — — — —		
CPREY	6.0	9.01
BPREY	2.0	6.48
FPREY	0.5	1.35

Stochastic Encounter Rate.—Table 3 gives the mean, standard deviation, and range for the foraging efficiencies at each diet breadth for three runs of the stochastic encounter rate simulation. Each run iterated the E/T calculation 100 times. The mean and standard deviation are graphed using the Z-score model in Fig. 2. The congruence among runs suggests that 100 iterations is an adequate sample.

It is evident in Figure 2 that the stochastic optimum and the deterministic optimum (db = 3) are the same for a wide range of R_{min} values, roughly 6 to 16. If the minimum tolerable intake is strongly below the expected, then the optimal (risk-sensitive) diet breadth will expand somewhat; if strongly above, then it will somewhat contract. These actions represent variance-averse and variance-prone behavior, respectively. Table 3 shows the results in more detail. Note that the range of E/T values is quite wide at all diet breadths. There is no choice available to this organism which can prevent it from experiencing some foraging intervals of only about 3 kcal/min net acquisition rate.

Three additional features of this simulation can be cited. First, while variance diminishes continuously from db = 1 to db = 6, the low end of the range of E/T values is highest at db = 4. This reinforces the observation that a risk-sensitive forager with a low R_{min} will increase its diet breadth above (but not much above) the deterministic optimum. Second, the distribution of foraging efficiencies over the 100 iterations is non-normal for some of the cases. Thus, it cannot always be assumed (see Stephens and Charnov 1982) that the distribution of stochastic food rewards will approach normality. Finally, at low diet breadths, the mean of the E/T values for each stochastic run is below that for the deterministic solution. This is a consequence of the fact that search costs

TABLE 3.—*Diet choice outcomes for the stochastic encounter rate simulations.*

	Foraging efficiency (E/T) by diet breadth for ranked prey items, 1 to 6					
Stochastic Model[1]	1	2	3	4	5	6
Run #1:						
Mean	1.26**	6.96**	9.35*	8.74*	6.99**	1.39**
s.d.	4.60	3.83	2.93	2.51	2.52	0.41
Range	(—2.00,12.65)	(—0.03,15.82)	(3.94,16.15)	(4.52,16.01)	(2.78,13.65)	(0.75,3.16)
Run #2:						
Mean	1.12**	6.80**	9.39	8.78	6.95	1.47**
s.d.	5.28	4.86	3.56	2.98	2.98	0.56
Range	(—2.00,17.12)	(—2.00,19.90)	(3.10,19.37)	(3.10,18.02)	(2.47,18.02)	(0.72,3.76)
Run #3:						
Mean	1.15**	7.04*	9.63	8.95	7.07*	1.43**
s.d.	4.67	4.00	2.88	2.44	2.59	0.45
Range	(—2.00,12.65)	(—2.00,17.11)	(2.84,17.09)	(3.78,14.87)	(3.04,14.86)	(0.72,3.73)
Deterministic Model[2]						
Value of E/T	1.85	7.63	9.94	9.01	6.48	1.35

[1]Parameter characteristics of prey and foragers as in Table 1, with prey density ($C/^*_i$) given as a stochastic poisson variate(λ_i = average prey density, d_i) over 100 sample iterations.

[2]See Table 2.

Kolmogorov-Smirnov test for a normal distribution; ** h_o (= normality) rejected, $p < .01$; * h_o rejected, $< .05$.

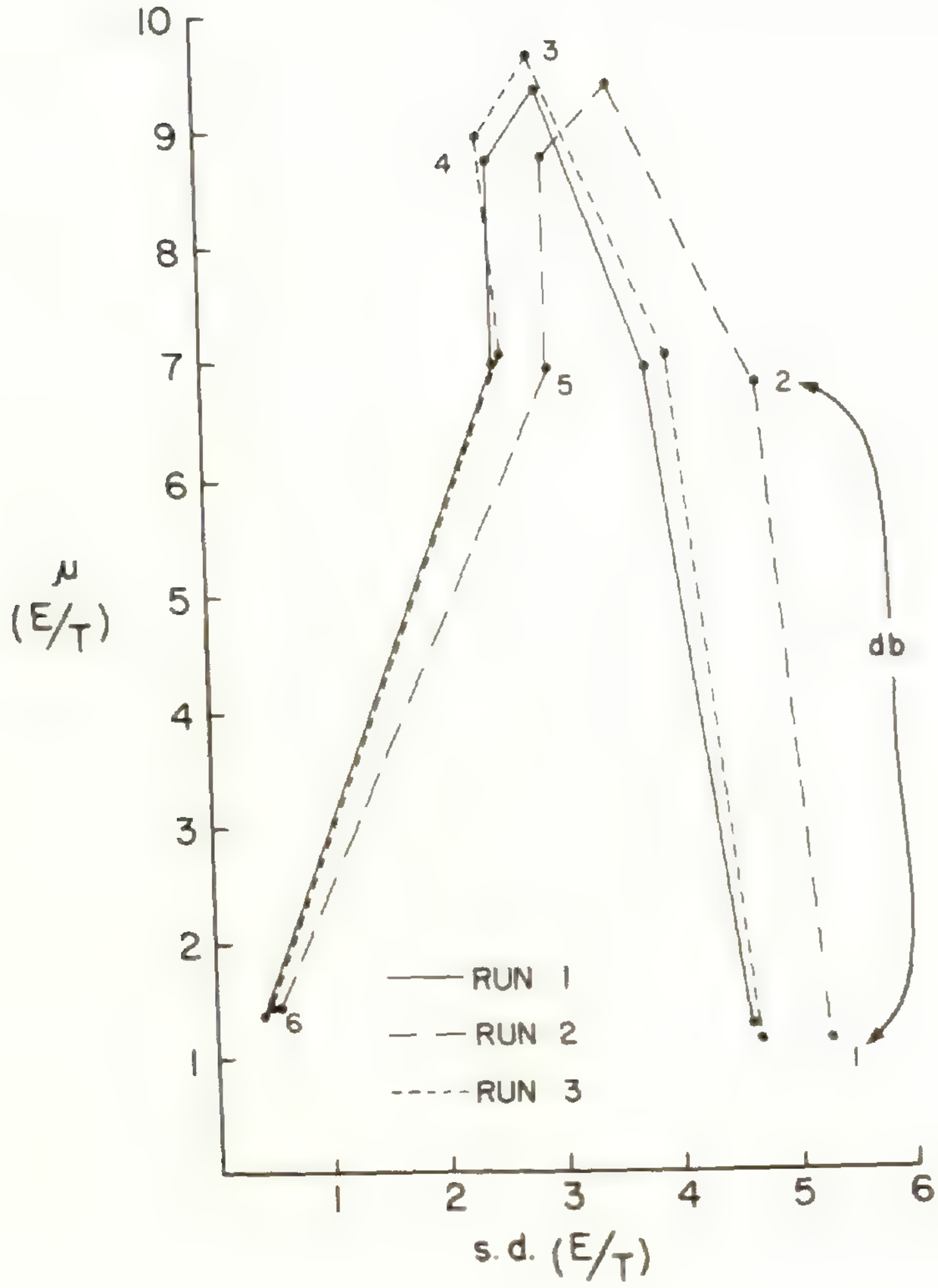

FIG. 2.—Mean/s.d. pairs at various diet breadths for the stochastic encounter rate simulations (db = diet breadth).

($-C_s$) are charged to intervals which happen to be empty of prey. Failure to encounter an acceptable prey type in a foraging interval is more likely for narrow diet breadths and rare prey.

This last observation represents a case in which the predicted value of an averaging (deterministic) model does not match the expected (or mean) value of its stochastic analog (see Poole 1979).

Stochastic Pursuit.—Using the same input parameters (Table 1), three runs of 100 iterations each were simulated in which pursuit time was a random normal variate. The results (Fig. 3) can be interpreted to indicate that the optimal choice of a risk-sensitive forager is again strongly convergent with the deterministic, rate-maximizing solution (db = 3). However, a risk-sensitive forager with a low R_{min} will, in this case, reduce its diet breadth, whereas that with a relatively high R_{min} will expand its selection of prey. These responses are opposite to those deriving from the stochastic encounter simulation.

Table 4 presents the results in greater detail. The range of E/T values at a particular diet breadth is not large. At db = 3 this organism can expect a net intake of at least 8 kcal/min in a stochastic foraging interval. Three additional observations are pertinent. First, while the standard deviation steadily increases from db = 1 to db = 5, in each run the minimum end of the range reaches a peak at db = 3. An organism sensitive to this minimum value will not stray from db = 3. Second, the distribution of foraging

TABLE 4.—*Diet choice outcomes for the stochastic pursuit time simulations.*

	Foraging efficiency (E/T) by diet breadth for ranked prey items, 1 to 6					
Stochastic Model[1]	1	2	3	4	5	6
Run #1:						
Mean	1.87**	7.67	9.98	9.11	6.60	1.38
s.d.	0.09	0.28	0.41	0.64	1.04	0.21
Range	(1.65,2.08)	(6.91,8.30)	(8.92,10.85)	(7.85,10.89)	(4.57,10.43)	(1.01,1.86)
Run #2:						
Mean	1.86**	7.66	9.96	8.99*	6.45	1.36*
s.d.	0.10	0.32	0.47	0.72	0.96	0.21
Range	(1.60,2.13)	(6.92,8.82)	(8.57,11.49)	(7.54,10.54)	(4.23,9.27)	(0.89,1.90)
Run #3:						
Mean	1.86**	7.60*	9.90	8.86	6.46	1.39
s.d.	0.10	0.29	0.41	0.70	0.89	0.24
Range	(1.60,2.08)	(7.13,8.62)	(8.93,11.37)	7.04,10.82)	(4.29,8.74)	(0.91,1.92)
Deterministic Model[2]						
Value of E/T	1.85	7.63	9.94	9.01	6.48	1.35

[1]Parameter characteristics of predator and prey as in Table 1, with prey pursuit times (t*i) given as a random normal variate (u = average prey density, s.d. = 0.25u) over 100 sample iterations.

[2]See Table 2.

**h_o rejected $p < .01$; *h_o rejected, $p < .05$; Kolmogorov-Smirnov test for a normal distribution.

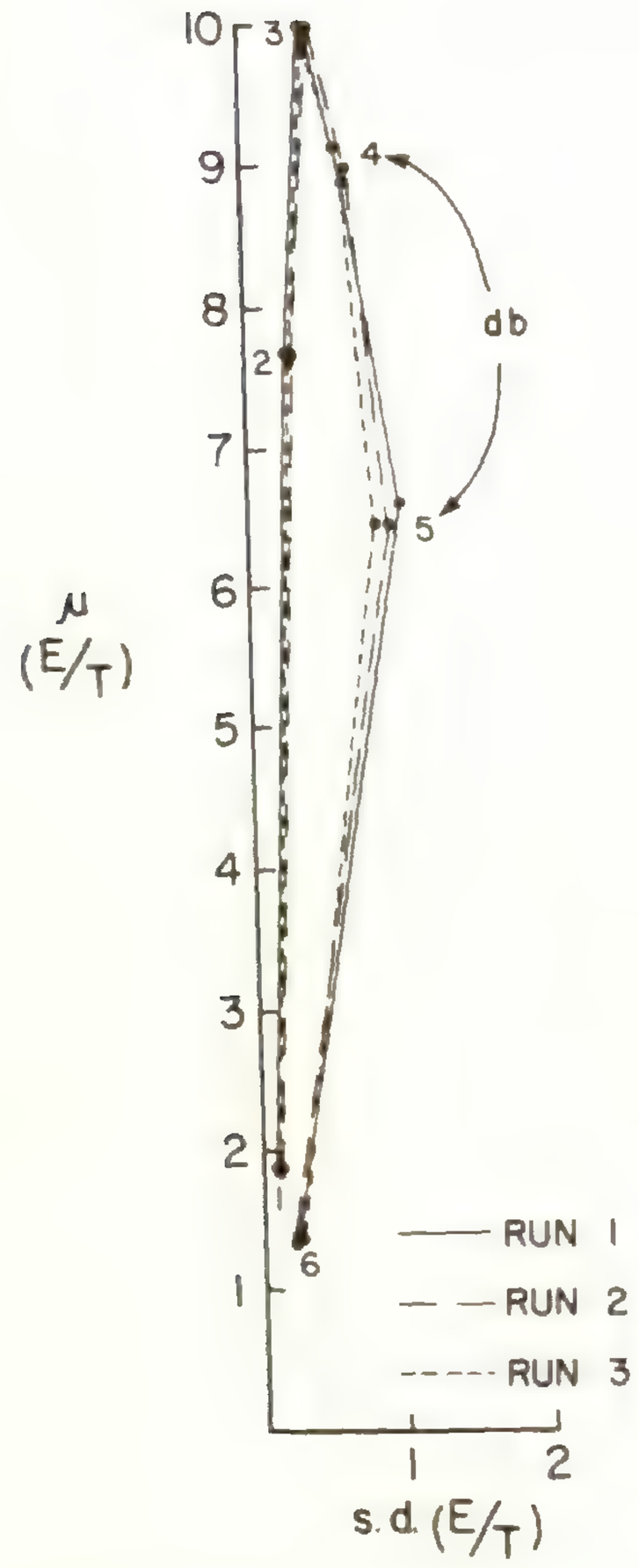

FIG. 3.—Mean/s.d. pairs at various diet breadths for the stochastic pursuit time simulations (db = diet breadth).

efficiencies (E/T) at each diet breadth deviates from normality in relatively few instances. Finally, the mean for the stochastic runs matches closely that for the deterministic model. This is due to the symmetry of the normal distribution and the absence of uncompensated costs (i.e., all pursued prey are assumed to be caught, sooner or later).

Random Encounter Rate and Pursuit Time.—The results for the combined model are shown in Fig. 4. Because the qualitative outcome is similar to that for Fig. 2, the interpretation applied to that case fits this one as well. For the parameters used, the search component generally swamps that due to pursuit. Although it is not readily evident in Fig. 4, the combined model should result in a relative narrowing of the (more or less) parabolic set of mean/s.d. pairs. This is because the smaller pursuit variances (db = 1, 2 and 3) are added to the larger search variances, and the larger pursuit variances (db = 4, 5 and 6) are added to the smaller search variances. The result enhances the convergence of the deterministic and stochastic outcome. This qualitative result—combining the two sources of variance actually reduces the divergence between risk-sensitive and efficiency-maximizing diet choices—is probably more significant than the precise quantitative outcome.

Table 5 gives the numerical results for the combined model. They are rather like those of Table 3: (i) the stochastic means at narrow diet breadths are below those for the deterministic case; and (ii) the peak minimum value encountered is at db = 4 in

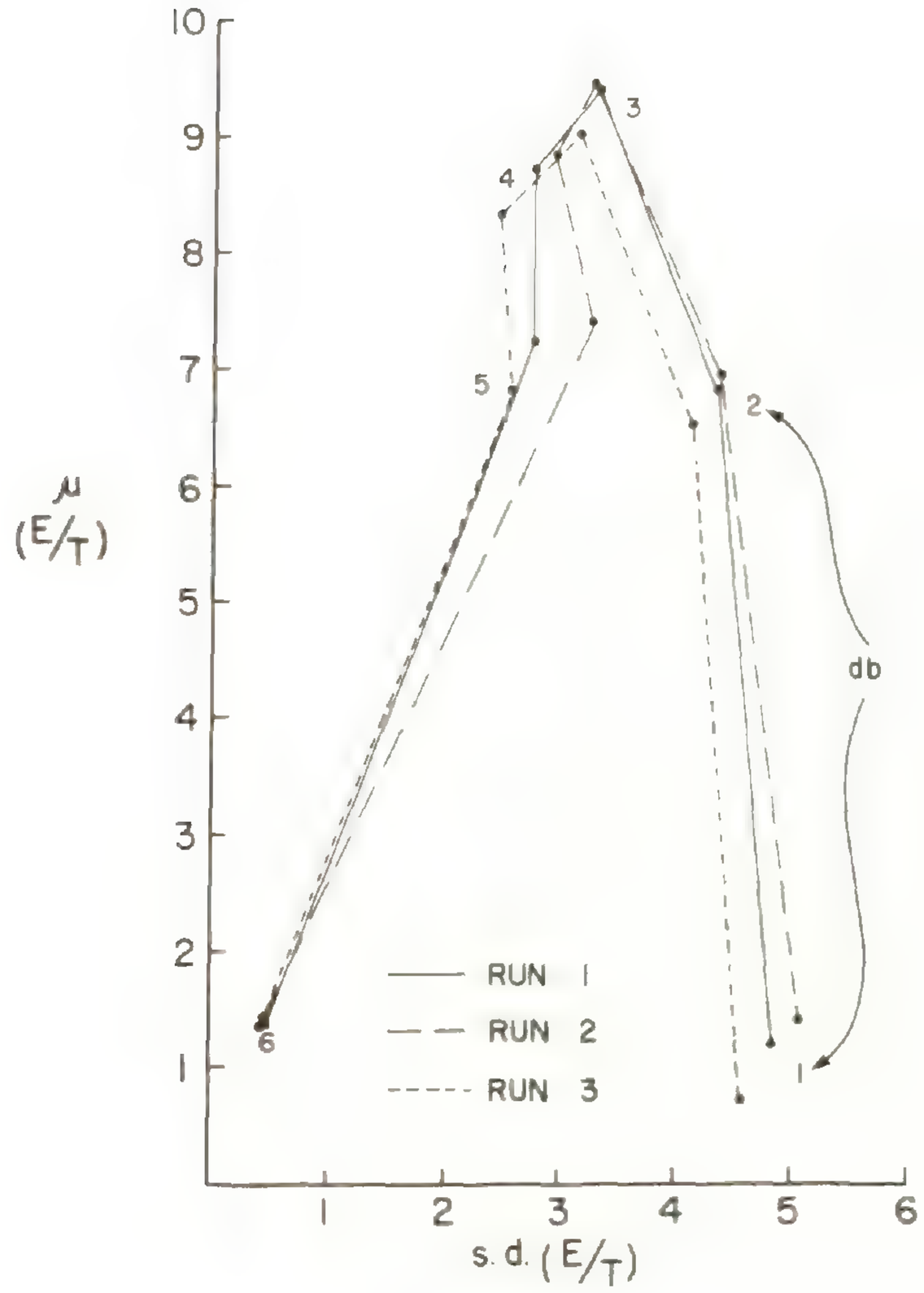

FIG. 4.—Mean/s.d. pairs at various diet breadths for the combined encounter rate and pursuit time simulations (db = diet breadth).

two runs and is equivalent at db = 3 or 4 in one run. Compared to Table 3, fewer of the distributions are non-normal. Adding an independent source of normal variation (stochastic pursuit times) should have this effect.

Even at db = 3 (optimal over a wide range of conditions), this organism will face some foraging intervals in which intake is only slightly above 2 kcal/min (those instances in which the few prey encountered are difficult to capture). For a db = 2 or 3, at least some foraging intervals will generate an intake of around 20 kcal/min, a 10 fold increase. Typically, both the range and standard deviation are larger in this combined model than for the stochastic encounter rate alone (compare Table 5 with Table 3), as is expected.

DISCUSSION

Rate-sensitive and risk-sensitive foragers.—For the marginal value theorem, Stephens and Charnov (1982) show that the optimal risk-sensitive choice in an unpredictable environment is similar to the rate-maximizing choice in a deterministic environment of averaged values. The risk-sensitive organism in positive energy balance somewhat increases patch residence time; that in negative energy balance somewhat reduces it. The convergence of the stochastic and deterministic optima is noteworthy. It allows us to retain the analytic simplicity of the original model, while appreciating its enhanced generality and realism. Stephens and Charnov observed that "there may be natural relationships between mean and variance which make solutions to the problems

TABLE 5.—*Diet choice outcomes for the combined (stochastic encounter rates and pursuit times) model.*

	Foraging efficiency (E/T) by diet breadth for ranked prey items, 1 to 6					
Stochastic Model[1]	1	2	3	4	5	6
Run #1:						
Mean	1.20**	6.82	9.41	8.73	7.22	1.39
s.d.	4.84	4.35	3.30	2.75	2.77	0.48
Range	(—2.00,18.69)	(—2.00,20.37)	(2.19,19.97)	(3.06,17.07)	(2.12,17.07)	(0.56,3.06)
Run #2:						
Mean	1.40**	6.93**	9.44	8.87**	7.42**	1.42
s.d.	5.05	4.35	3.26	2.93	3.29	0.46
Range	(—2.00,18.69)	(—0.04,20.04)	(2.26,19.85)	(3.76,19.85)	(2.61,19.85)	(0.60,2.94)
Run #3:						
Mean	0.72**	6.52	9.04*	8.33	6.80	1.35
s.d.	4.58	4.13	3.15	2.44	2.58	0.44
Range	(—2.00,13.58)	(—2.00,16.26)	(2.36,16.39)	(2.36,14.10)	(2.10,14.06)	(0.62,2.93)
Deterministic Model[2]						
Value of E/T	1.85	7.63	9.94	9.01	6.48	1.35

[1]Parameter characteristics of prey and forager as in Table 1, with prey density ($C/^*_i$) given as a stochastic poisson variate (λ_i = average prey density) and pursuit time (t^*_i) given as a normal stochastic variate (u = average prey density, s.d. = 0.25u) over 100 sample iterations.

[2]See Table 2.

**h_o rejected, $p < .01$; *h_o rejected, $p < .05$; Kolmogorov-Smirnov test for a normal distribution.

of 'energy maximization' and 'minimization of the probability of starvation' similar" (1982:251). However, given only one concrete example (the marginal value theorem) they were hesitant to claim generality for this possibility.

The results obtained here strengthen the case for such a "natural relationship". In each of the three simulations—stochastic encounter rate, stochastic pursuit time, and the combined case—the risk-minimizing and efficiency-maximizing solutions were the same for a wide range of minimum intakes (R_{min}). Faced with stochastic encounter rates, the simulated forager strongly in positive energy balance ($R_{min} \langle\langle$ net acquisition rate) will *increase* diet breadth somewhat. Faced with stochastic pursuit times, the forager strongly in positive energy balance ($R_{min} \langle\langle$ net acquisition rate) will *reduce* diet breadth somewhat. The converse predictions hold for the case of negative energy balance. The predictions of the combined model are like those of the stochastic encounter simulation.

If an inverted "U" concave toward the x-axis found in this simulation represents the general case for diet breadth choices, then the ranked set of prey species available will occupy a counter-clockwise sequence of positions along the curve (Fig. 5). For an optimal diet which includes all available resource items (a to e), a risk-sensitive forager in positive energy balance is constrained to the efficiency-maximizing choice. There is no species to add, no increment of risk-minimizing to gain, even at the cost of efficiency. Likewise, if the optimal deterministic diet includes only one prey type from a potential set (e to i), then a forager in negative energy balance is constrained to the deterministic optimum. A complete generalist doing well, or a complete specialist doing poorly, can each do no better than the rate-maximizing choice.

It is also conceivable that several of the potential prey would rest at the peak of the curve (between d and f), generating a relatively broad but shallow segment in the set of mean/s.d. pairs. In this case, a small change in forager energy balance, one within a limited portion of the R_{min} range, would have a more dramatic impact on diet breadth.

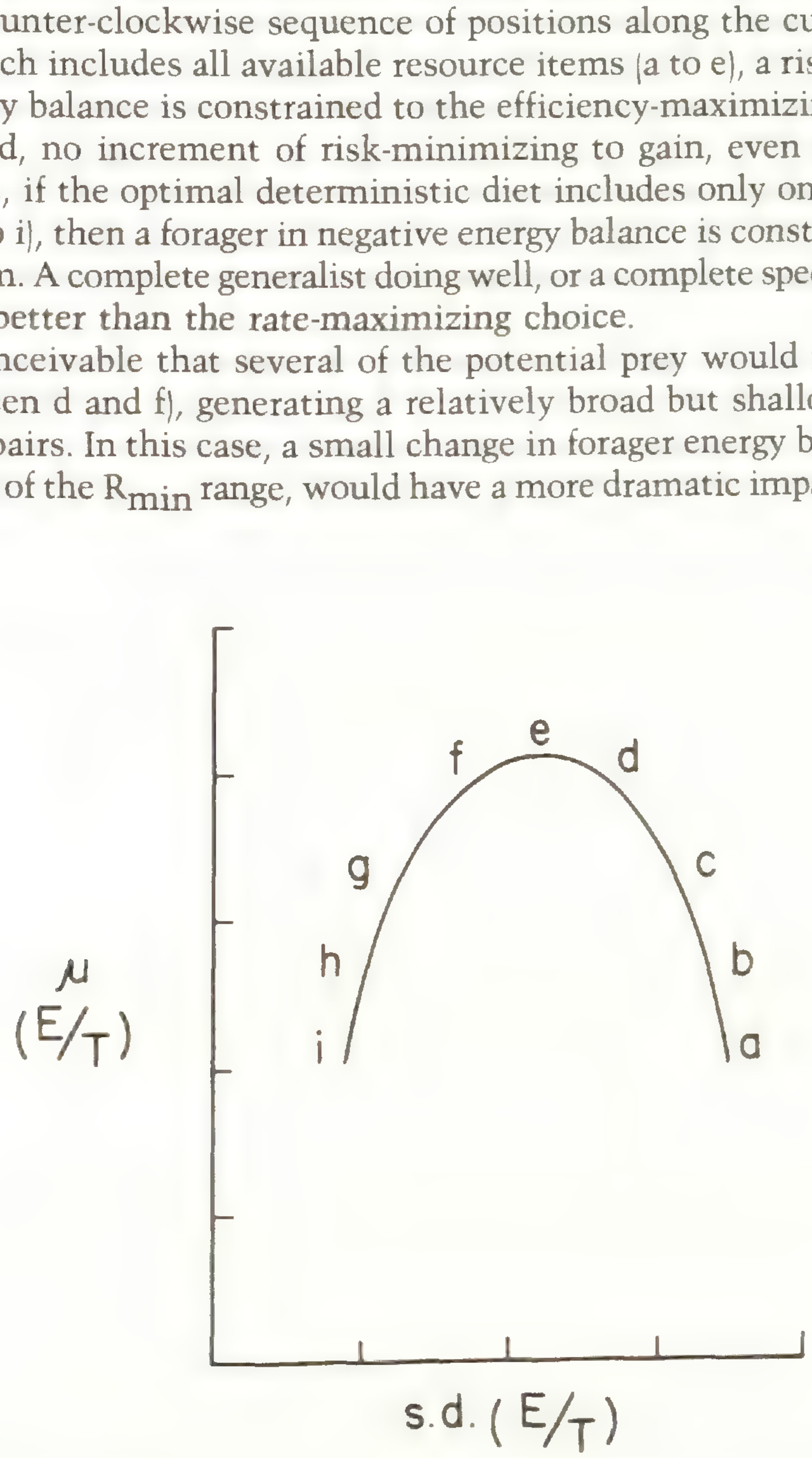

FIG. 5.—Generalized mean/s.d. set for diet choices in an unpredictable environment.

Taken to its limit this would approach the extreme variance rule (Stephens and Charnov 1982) as the set became linear. This situation probably is rare, as it would require that each sequentially added prey type have precisely those qualities which produce very little change in the average net acquisition rate of foraging while simultaneously producing a large reduction in the s.d. of the net acquisition rate.

These special cases reinforce the point made earlier: it may often be the case that the deterministic and stochastic optima will converge. Doing well seems to be the best way to not come up short, even in an unpredictable environment.

Disadvantages and Advantages of Simulation.—Simulation is a useful means of generating solutions to equations difficult to solve more directly. However, a simulation approach produces data that are not easily generalized, an important disadvantage. Without extensive experimentation, it is difficult to determine the sensitivity of the results to the peculiarities of the input parameters. Despite this limitation, the technique does produce actual values for a given set of inputs, and these may yield insights not readily evident in the more general solutions reached by mathematical derivation.

In the present instance, the simulation reveals details that might be missed in a purely qualitative application of the Z-score model. For instance, Fig. 4 indicates that a risk-sensitive forager with an R_{min} in the range of 5 to 17 kcal/min will choose a db = 3. In a qualitative interpretation, a change in R_{min} from 17 toward 5 might be interpreted to "increase" the optimal diet breadth of a risk-sensitive forager, but in fact, the number of prey items will not change. There is a broad plateau of stability for db = 3. Real prey choices are not continuously distributed along the curve; they are discrete and if limited in number, may be widely spaced.

How good are these optimal risk-sensitive solutions? Aggregating runs No. 1, 2 and 3 (Fig. 4) to give a total of 300 foraging intervals shows that db = 3 has intakes exceeding 17 kcal/min in 2 instances (0.67%) while a diet breadth of 2 items exceeds this value in only 3 cases (1.00%). An organism with an $R_{min} \rangle$ 17 increases its odds of avoiding starvation by 50% if it drops from 3 to 2 prey in the diet, but the chance of obtaining a net acquisition rate of 17 or greater is still very small. Similarly, for an R_{min} of 5, a diet breadth of 3 generates 20 intervals with shortfalls (6.67%), while a db = 4 decreases this to 16 such cases (5.3%), again only a small improvement. In effect, the best solution to a bad situation ($R_{min} \rangle\rangle$ net acquisition rate) might rarely be good enough, and the best solution for a risk-sensitive organism in strong positive energy balance ($R_{min} \langle\langle$ net acquisition rate) might still be relatively precarious. No diet choice can prevent this organism from experiencing the rare interval below 3 kcal/min, nor help it above 21 kcal/min. The actual range of the food reward over some finite set of intervals thus is a salient consideration.

There is a semantic issue here worth noting because it is occasionally confused by critics of optimal foraging theory. The optimal solution in foraging theory is the *best possible* for a constrained situation and set of choices. It does not mean the most desirable outcome. Put differently, to minimize risk is not necessarily to avoid it. There is, after all, neither fitness nor consolation in making an optimal choice if the reward is still starvation, as it might well be in stochastic circumstances.

In actuality, the rare occurrence of a decidedly low value (or high value) may be less important than a series of intervals with low values. Extremely poor capture rates may be infrequent or perhaps of relatively short duration, and the unlikely and unusually poor interval tolerable. This suggests that distributional statistics are more important than range endpoints. A satisfactory (Step 1) goal may be to avoid the accumulated effect of poor foraging intervals which are frequent enough to clump together.

Stephens and Charnov (1982) showed that in the general case the foof reward distribution will approach normality. In the present simulation this was not always

true, probably because of the dominating effect of the search component of the model, hence of the poisson distribution. This distribution becomes highly skewed (right) as the mean drops from about 5 into fractional values, the range characteristic of most of the prey types used in this simulation (see Table 1). In fact, all of the distributions in Tables 3 and 5 are skewed to the right (positive g_1). This suggests that it is important to analyze the actual shape of the E/T distribution before advancing with inferences based on the Z-score technique (Fig. 1), if any of the prey types are rarely encountered during the foraging interval.

CONCLUSION

Because the environment of a species is always more or less stochastic it is important to know if foraging strategies must consider unpredictability in the capture rates associated with various choices. It is a boon to the forager, and to the human and evolutionary ecologist, that the structure of food choices and patch residence time appear to generate similar strategies for rate-maximizing and risk-minimizing goals. For the forager, one choice generally will cover the concurrent needs to maximize efficiency and minimize risk; for the analyst, the diet choice predicted by the simple efficiency-maximizing model also will approximate the risk-minimizing solution. In effect, the risk-sensitive forager is constrained to be highly rate-sensitive by the relationship between the mean and variance of energy capture rates. Empirical examination of this relationship should be a high priority in foraging studies.

The forager making a risk-sensitive choice in a stochastic environment still faces a rather large variation in intake from one interval to the next. Viewing nature through the model makes commonplace survival seem extraordinary. Several considerations may temper this conclusion: (a) minimum requirements (R_{min}) generally may be low relative to expected intake (average net acquisition rate); (b) the critical period may be long and not clearly bounded by a fitness threshold, perhaps because endogenous reserves or stored food are available (in fact, the concepts of positive and negative energy balance imply some ability to "average over" poor intervals); or, (c) there may be considerably more predictability in the environment that in simulations based on randomizing poisson and normal distributions. Each of these possibilities will lessen the impact of the abstract, statistical vagaries of the simulated food quest. A fourth possibility, perhaps especially important for hominids and prehistoric or extant hunter-gatherers, is central-place sharing by foragers who nonetheless hunt and gather independently (Winterhalder 1986b).

ACKNOWLEDGMENTS

The comments and criticisms of Paul Gardner, Joe Laferriere, Marc Mangel, Eric Smith and Dave Stephens are acknowledged with gratitude. Gayle Fritz provided able assistance with the statistical analyses.

NOTES

[1]The programming and analyses were done on an Apple II + microcomputer in Apple Pascal by Sara DeGraff, using the pseudo-random number generator available in the language software. The distributions of prey densities and pursuit times were checked (by Chi-square) and none differed significantly from the expected poisson and normal frequencies. Statistical analysis of the distributions of foraging efficiences (Tables 3, 4, and 5) were done on SAS.

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NEWS and COMMENTS

SOCIETY OF ETHNOBIOLOGY NEWS

Tenth Annual Conference

The 10th Annual Conference of the Society of Ethnobiology will be held March 5-8 1987 at the Florida State Museum, University of Florida. Papers are invited on the following and related topics: cultural ecology, plant and animal domestication, ethnozoology, zooarchaeology, ethnobotany, archaeobotany, palynology, ethnopharmacology, human diet and nutrition, folk taxonomy. For further information please write to Elizabeth S. Wing, Florida State Museum, Gainesville, FL 32611 / (904) 392-1721.

Barbara Lawrence Prize Announced

The Society will award a prize in honor of Barbara Lawrence for the best paper submitted by a student for presentation at the 10th Annual Meeting. The competition is open to any member who considers themself a student and has not held the PhD degree at the end of the preceding summer session. The paper can be presented in an oral or a poster session and will be considered for publication in the *Journal of Ethnobiology*.

Manuscripts submitted for this competition should be single authored only; joint efforts will not be considered. Manuscripts are judged solely on quality, originality, and presentation of research. They should follow the *Journal of Ethnobiology* format and should be sufficiently precise and documented to enable the reviewing committee to judge their merits. Manuscripts are limited to eight doubled-spaced, typed pages, including a required abstract but excluding copies of figures, tables, and references.

Please include a cover letter indicating the you are a Society member and meet the criteria listed above and send it and your paper to the 10th Annual Conference committee, Florida State Museum, Gainesville, FL 32611.

ANNOUNCEMENTS and REQUESTS

The Society for Economic Botany announces the appointment of a new editor of the Society's journal *Economic Botany*. John Thieret replaces Oswald Tippo as of June 1986. The Society's 27th Annual Meeting was held 13-16 June this year at the New York Botanical Garden in the Bronx, New York. Highlights included a symposium on palms and a keynote address by this year's Distinguished Economic Botanist, Efraim Hernandez-Xolocotzi. The 28th Meeting is scheduled for Chicago 22-25 June 1987, with a symposium on traditional medicine as focal point. Contact Susan Verhoek, Society president, Department of Biology, Lebanon Valley College, Annville, PA 17003, for more information.

The School of American Research sponsored a seminar on "Bone Chemistry and Past Behavior" chaired by T. Douglas Price, 3-7 March 1986. The chemical analysis of bones reveals information about diet, nutritional status, disease, even social status. Papers are to be published in the School's Advanced Seminar Series. The School of American Research also wishes to announce a 1 December 1986 deadline for applications to their anthropology resident scholars' fellowship program. Contact Susan Bodenstein at the school, P.O. Box 2188, Santa Fe, NM 87504, for details.

The Laboratorio de Etnobiologia, Universidade Federal de Maranhão announces plans for the First International Congress of Ethnobiology, 15-21 June 1988 at the University in São Luiz, Maranhão, Brazil. The six-day congress will be divided into three parts: (1)

three days devoted to specialized areas of ethnobiological research, (2) two days for interdisciplinary discussions arranged around geographic interests and area studies, and (3) a final day reserved for presentations of applied ethnobiological projects and discussions of the potential of ethnobiological research for the improvement of world conditions. Organized symposia are invited. for additional information please write: Prof. Dr. Darrell Addison Posey, Coordinator, Laboratorio de Etnobiologia, Largo dos Amores, 21, Universidade Federal do Maranhao, 65,000 Sao Luiz, Maranhao (Brazil) or phone 098-221-1796.

Celestial Seasonings Laboratory requests assistance in building a collection of herbarium voucher specimens and seeds. They are most interested in plants used in teas but are interested also in expanding their herbarium collections of toxic plants that may be adulterants in teas and in other economically valuable plants. For information contact Trish Flaster, Curator, at the Laboratory, 1780 55th St., Boulder, CO 80301, or call (303) 449-3779.

ETHNOBIOLOGY in the NEWS

Gentians outlawed in Tennessee:

The Tennessee House voted 95 to 0 this past February to make it a misdemeanor to deliver, sell, or possess on school grounds the seeds of *Gentiana lutea*, equated in the legislation with "jimson weed." Not so, says Nashville botanist Milo Guthrie. The Latin designation for jimson weed is *Datura stramonium*. What the bill bans is a harmless plant bearing yellow flowers that grows in the Alps and Pyrenees. The legislature wanted to outlaw jimson weed seeds because chewing them produces a hallucinogenic effect and has made several Nashville students ill recently. The bill's sponsor, State Rep. Shelby Rhinehart, said it was drafted by the legislature's legal staff and can be fixed by the state Senate. "I just told them to outlaw jimson weed. I didn't tell them to put that Latin in there," he said. (Adapted from an AP report in the *Washington Post* on 17 February 1986.)

Robert Bye, who sent the clipping, suggests that the confusion may have arisen from a phonological conflation of "jimson" with "gentian" in the local dialect. Bye recommends that the lawmakers consult G. A. Mead's 1970 note "On the Improper Usage of Common Names when Giving Botanical Data," *American Antiquity* 35:108-109.

Are Fungi the Stradivari Violin's Secret Ingredient?

A Seattle P.I. Science Brief this spring reported that the secret of the pure tone of Stradavari violins may be due to the effect of a microscopic fungus that grows within the cells of the wood used to make these classic instruments. Microscopic examinations of the samples showed that traces of fungi—which could have grown only through water immersion—had altered the shape of the wood cells, according to Joseph Nagyvary, Texas A&M professor of biophysics and biochemistry. Water fungi eat gummy material in the wood and make it lighter and drier, he said. They also force the cells to separate and loosen up. Nagyvary traveled to Europe and examined old shipping records stored in villages and monasteries. The records showed that logs had been sent downstream along the rivers leading from the Tyrolean Alps, where most of the wood was cut, to the Italian towns where the instruments were made.

Haitian Pig Roast:

A. Oppenheimer reports from Port-au-Prince (Seattle *Post-Intelligencer* of 15 June 1986) that peasants are protesting the consequences of a $22 million joint U.S.-Haitian program put into effect in 1984 to improve Haitian pig herds. Under the direction of the International Institute of Agricultural Cooperation Haiti's 1.2 million pig popula-

tion was exterminated in 1984 to be replaced by U.S.-bred pigs. Western experts had characterized the Haitian pigs as "a degenerate species *(sic)*, a pig that hadn't been properly fed in 100 generations," a long-legged breed that "roamed freely" and subsisted on "orange peels, mango seeds, and garbage." These pigs were infected with African swine fever. The new breed of pigs, however, requires imported feed, concrete-floored stalls, expensive vaccines, and is too short of leg to walk to market. Critics of the program argue that the peasants, who constitute 70% of Haiti's population, traditionally relied on pigs as an investment, a way of saving for their children's education. The delicate new breed cannot be properly cared for and often dies prematurely, forcing Haiti's impoverished farmers to sell their land.

Zapotec and High Tech:

The *Mexico City News* of 27 January 1986 carried an account of Gary Martin's efforts in collaboration with local herbalists in Oaxaca, Mexico, to establish demonstration medicinal herb gardens and medicinal herb "libraries" in rural village centers, using a micro-computer to collate information on medicinal applications of Oaxaca's some 4000 plant species by indigenous curers. For further information contact Martin at G.A.D.E., A.C., Apartado Postal 379, 68000 Oaxaca, Oaxaca, Mexico.

GRUPO ETNOBOTANICO LATINO AMERICANO (GELA) CHARTERED

During the Fourth Latin American Botanical Congress, which was celebrated in Medellin, Colombia, from June 29 to July 4, 1986, the "Grupo Etnobotanico Latino Americano" (GELA) was chartered as a section of the Latin American Botanical Association. This council consists of one representative from each country in Latin America and one from the United States, including its coordinating committee. Mexico City, D.F., is the seat of the first coordinating committee, which is chaired by Javier Caballero, with Victor Manuel Toledo, Monserrat Gispert, Arturo Gómez-Pompa and Armando Contreras as members. The representatives from each of the other countries, at this point, include Anthony Anderson (Brasil), Eduino Carbono (Colombia), Sonia Lagos (Honduras), David Diaz-Miranda and Maximina Monasterio (Venezuela), Gary Martin and Arturo Gómez-Pompa (USA).

The principal function of GELA is to promote scientific exchange between different researchers who are actively working in the field of ethnobotany. This council will use the Bulletin of the Latin American Botanical Association as its medium of information dissemination.

The primary tasks of the Coordinating Committee of GELA are the following:

1. Form a directory of ethnobotanists from Latin America, the U.S. and other parts of the world, who are active in researching this topic. To move toward this goal, it will soon distribute a registration form to be entered into a computerized data base.
2. Form a bibliographic clearinghouse of publications by those researchers included in the directory, to be placed in the care of the Secretary of the Library of the Mexican Botanical Society. This library is actually located within the facilities of the Botanical Garden of the National Autonomous University of Mexico. This collection will be available to researchers of any country, via a photocopying service.
3. Promote the organization of a meeting of Latin American ethnobotanists just prior to the celebration of the next Latin American Botanical Congress. This meeting will be celebrated in Mexico in 1988.

For more information of GELA, write to: Javier Caballero, Jardín Botánico, UNAM, Ciudad Universitaria, Coyoacan 04510 México D.F. *[Translated by Gary Nabhan].*

Each spring semester the San Diego Natural History Museum and the Museum of Man jointly sponsor a one-day ethnobiology seminar honoring the late Dr. Raymond M. Gilmore (see J. Ethnobiol. 4:97-99, 1984). The first, in 1985, featured Dr. Johanes Wilbert on the cosmology of the Warao Indians of Venezuela. The following year Dr. Thomas R. Van Devender spoke on changes in climate and Sonoran Desert begetation during the last 20 thousand years based on his studies of packrat middens.

Our next seminar, scheduled for Saturday, 7 February 1987, at the Natural History Museum, will be presented by a panel actively researching *Cucurbita* spp. (pumpkins and squash). Dr. Thomas W. Whitaker, Ms. Laura Merrick, and Dr. Gary Nabhan will present archaeologic, ethnographic, and genetic aspects of this segment of the traditional New World corn-beans-squash trinity.

Society of Ethnobiology
1987
Annual Meeting Notice

The 10th Annual Meeting
of the
Society of Ethnobiology
will be held on
March 5-8, 1987
at the
Florida State Museum
University of Florida

Please see
News & Comments
on page 225 for details

NOTICE TO AUTHORS

The *Journal of Ethnobiology* accepts papers on original research in ethnotaxonomy and folk classification, ethnobotany, ethnozoology, cultural ecology, plant domestication, zooarchaeology, archaeobotany, palynology, dendrochronology and ethnomedicine. Authors should follow the format for article organization and bibliographies from articles in this issue. All papers should be typed double-spaced with pica or elite type on 8½ x 11 inch paper with at least one inch margins on all sides. The ratio of tables and figures to text pages should not exceed 1:2-3. Tables should not duplicate material in either the text or graphs. All illustrations are considered figures and should be submitted reduced to a size which can be published within a journal page without further reduction. Photographs should be glossy prints of good contrast and sharpness with metric scales included when appropriate. All illustrations should have the author(s) name(s) written on the back with the figure number and a designation for the top of the figure. Legends for figures should be typed on a separate page at the end of the manuscript. Do not place footnotes at the bottom of the text pages; list these in order on a separate sheet at the end of the manuscript. Metric units should be used in all measurements. Type author(s) name(s) at the top left corner of each manuscript page; designate by handwritten notes in the left margin of manuscript pages where tables and graphs should appear.

If native language terminology is used as data, a consistent phonemic orthography should be employed, unless a practical alphabet or a more narrow phonetic transcription is justified. A brief characterization of this orthography and of the phonemix inventory of the language(s) described should be given in an initial note. To increase readability native terms should be indicated as ***bold-face italics*** to contrast with the normal use of *italic* type for foreign terms, such as latin binomials. If necessary, the distinction between lexical *glosses*, i.e., English language approximations of a term's referential meaning, and precise English equivalents or definitions should be indicated by enclosing the gloss in single quotation marks.

Authors must submit two copies of their manuscript plus the original copy and original figures. Papers not submitted in the correct format will be returned to the author. Submit your manuscripts to:

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NEWS AND COMMENTS

Individuals with information for the "News and Comments" section of the *Journal* should submit all appropriate material to Eugene Hunn, Department of Anthropology, DH-05, University of Washington, Seattle, Washington 98195.

BOOK REVIEWS

We welcome suggestions on books to review or actual reviews from the readership of the *Journal*. Please send suggestions, comments, or reviews to Charles H. Miksicek, Office of Arid Lands Studies, University of Arizona, Tucson, Arizona 85721.

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Journal of Ethnobiology

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Journal of Ethnobiology

VOLUME 6, NUMBER 2 WINTER 1986

Recently, in a telephone conversation with Linda Desmond, typesetter for the *Journal*, I mentioned having difficulty in deciding upon a topic for this issue. She volunteered that she liked the column on apple butter and added "Give us more apple butter." (See Volume 4, Number 2). That article had been inspired by the holiday season, and having just distributed petite fruit cakes as holiday gifts, I'll tell you about the recent evolution of the recipe I used.

Let's suppose your situation is this. The *idea* of fruit cake appeals to you; it is, afterall, traditional holiday fare and they *are* festive in appearance. You and your family have never enjoyed the heavy, moist texture of the fruit cakes you've sampled, not even the blue ribbon winners at the county fair. You cannot justify the expense and effort of baking fruit cakes because of their festive appearance alone, so you have a dilemma. The easy solution is to firmly declare—as many do—that fruit cakes (or anything else you've decided you don't like) are overrated, aren't worth the effort, are too expensive, etc. and you might even look with disdain upon those who enjoy them. The more creative way, and happily for Society many also take this course, is to use your abilities as a domestic scientist: decide what you don't like in the fruit cakes you've sampled, identify those qualities you do like and be sure to emphasize those, invent innovations of your own and otherwise experiment. Since fruit cake is usually made only at holiday time, it may be several years before you have perfected a family tradition.

Let's further suppose you live on a farm in the Mid-west of the U.S.A. You have available, free for the taking, black walnuts, hickory nuts, hazel nuts, cherries, currants, etc. You also have a number of children who are just the right age to harvest these commodities and prepare them for baking (and, incidentally, in the case of black walnuts this involves a rather considerable amount of time and effort). And, enjoying the flavor of nuts, you decide to feature them, that is, your fruit cake will have more nuts than fruit.

But, what about the texture? Was there a reason, originally, for the moistness of the cake? Did it serve a function that isn't important to you? In what ways can you eliminate the unwanted heaviness? If increasing the amount of leavening isn't the course you want to pursue, do you have another option?

You realize, perhaps subconsciously, that doing something about the texture requires a re-thinking of the concept of the batter. You identify that it's essentially a combination of wet and dry ingredients. If one reduces the amount of batter and incorporates its two components separately, will this achieve the desired effect. So, you try it and discover that it does! You now have a fruit cake which is mostly nuts and fruit with just enough batter to hold the really tasty ingredients together. In effect, the nuts and fruits are glazed with the batter in the same manner, more or less, that puffed popcorn grains are stuck together with the sorghum molasses glaze in the popcorn balls for which you are so well known. You even wonder if this, subconsciously, is from whence the idea came.

As the years pass and the family matures, you lose that source of labor. You find it expedient to use purchased nuts and discover that almonds and Brazil nuts are wonderful too. Some of the children leave the area, even as far away as Arizona.

But hickory trees do not in sunny Arizona grow. Nor are hickory nuts or black walnuts readily available for purchase, so the traditional family recipe becomes modified with further substitutions: pecans for hickory nuts, English walnuts for black walnuts, raisins for currants, purchased candied fruit, etc.

Perhaps this anecdote of the evolution of a recipe for fruit cake will remind us that family traditions are modified with changes in circumstances. So, too, traditions within a larger cultural setting are far from static, changing with time and circumstances, as Jan Timbrook pointed out so well (see Volume 4, Number 2:143-144).

W.V.

J. Ethnobiol. 6(2):229-238 Winter 1986

RECOGNITION OF VARIABILITY IN WILD PLANTS BY INDIANS OF THE NORTHWEST AMAZON: AN ENIGMA

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"There is evidence that herbal doctors distinguish between different varieties, anatomical parts, stage of maturity (for all of which they have a vocabulary) and times of collection . . . This information may be very significant in terms of medical effectiveness of the remedy."

Abbot and Shimazu (1985)

I.

The Indians of the northwest Amazon—that region lying in Colombia and Ecuador and adjacent areas of Brazil and Peru—have an uncanny familiarity with the plants of the ambient vegetation and with their biodynamic activity. This knowledge and skill is obviously the result of millennia of intimacy with and utter dependence upon the flora, combined with the intense curiosity about natural phenomena that characterizes these people.

One of the enigmas that botanists have not yet had much success in understanding concerns the native's recognition of "kinds" or "varieties" of many of the wild species of their useful plants. The variants are so well established in the Indians' classifications that they usually have distinguishing native names.

This skill is manifest not only to those few of the 80,000 species native to the region that are economically important but is found in the aboriginal classification of a number of plants which apparently have little or no importance as utilitarian, ceremonial, magical or mythological species.

In most cases, it is botanically impossible to discern morphological differences on which subspecific taxonomic categories might be recognized. Yet often—nay, usually—an Indian can tell at once and frequently on sight and at a significant distance, without feeling, tasting, smelling, crushing, tearing or other physical manipulation, to which category a plant belongs. The "identification" of these "kinds" is indeed a complex interdisciplinary problem, but, while it is obviously of deep significance to the anthropologist and psychologist, it is of extraordinary importance to the botanist and phytochemist.

Little research on this fascinating aspect of ethnobotany seems to have been attempted. Most of the "explanations" offered are pure conjecture. It has been suggested that these recognized named "varieties" may be simply different parts of a large plant, sundry age forms or portions growing in shade, sun or under other environmental conditions. It is very probable—especially with food, medicinal, narcotic or toxic species—that some of these "varieties" may represent chemovars. But, if so, how can a native visually identify which chemovar it is and give it the name that his language has for that variant? I have tested the perspicacity of the Indians in this respect on many occasions and have rarely found them hesitant, doubtful or in error. And Indians of different tribes and living at appreciable distances from one another will identify these variants with amazing consistency.

There are no better examples of this aboriginal perspicacity than the stimulant of the westernmost Amazon of Colombia and Ecuador—***yoco,*** the sapindaceous vine *Paullinia Yoco* (Schultes 1942)—and the source of the western Amazon hallucinogenic drink variously known as ***ayahuasca, caapi, natema, pindé, yajé***—the malpighiaceous liana, *Banisteriopsis Caapi* (Schultes 1957).

Evaluation of field studies and studies of voucher specimens of these two major economic plants illustrate the enigma surrounding the aboriginal ability to identify "variants" of the species in their rich ambient vegetation.

The specimens cited are preserved in the Economic Herbarium of Oakes Ames and/or the Gray Herbarium, both at Harvard University, and/or in the Herbario Nacional de Colombia in Bogotá, Colombia.

II.

Paullinia Yoco *R. E. Schultes et Killip* in Bot. Mus. Leafl., Harvard Univ. 10 (1942) 302, t. xxvii.

Paullinia (Fig. 1) is a genus of the Sapindaceae comprising more than 180 species of lianas of tropical or subtropical America. One species, *P. Cupana* Kunth is cultivated in the Brazilian Amazon as the source of a caffeine-rich drink prepared from the seeds.

Anthropological writings and reports of travellers frequently made mention of ***yoco*** in the Colombian Putumayo. Material collected by the Belgian scientist Florent Claes in the late 1920s was chemically examined and found to contain caffeine. It was misidentified as *Paullinia scarlatina* Radlk., a species that does not occur in South America. In 1942, the liana was definitively identified and described as a new species of *Paullinia: P. Yoco* (Schultes 1942).

Eighty years ago, the Colombian writer Zerda Bayon reported that the Indians of the Comisariás del Caquetá and Putumayo distinguished various kinds of ***yoco,*** stating that "there is a ***yoco blanco*** ["white yoco"] and a ***yoco colorado*** ["red yoco"]. His specimens have long since been lost, and he did not attempt to explain the differences between these two named kinds of ***yoco.***

In 1942, when the binomial *Paullinia* Yoco was published, I commented as follows on the several named and recognized variants. "During my ethnobotanical studies in the Putumayo, I repeatedly questioned natives concerning the differences between ***yoco blanco*** and ***yoco colorado*** with conflicting replies. While it is true that the sap expressed from some stems makes a light chocolate-brown mixture when added to cold water, that from other stems makes a whitish milky mixture. Both taste the same, and both are equally effective as a stimulant. The Indians do not prefer one to the other. I find it impossible to distinguish botanically the liana which gives ***yoco blanco*** from that which yields ***yoco colorado,*** but the natives can identify them immediately by slashing the bark with a machete. I have noticed that ***yoco colorado*** nearly always is a much stouter and apparently older plant than ***yoco blanco.*** It is not possible that the differences are due to seasonal or soil conditions, for ***yoco blanco*** and ***yoco colorado*** grow side by side and can be collected at the same time" (Schultes 1942).

One of the recent collections *(Pinkley 380)* reports that ***totoa-yoko*** ("white yoco") "has more leche ('milk') than other types and is therefore the best type." *Klug 1955* indicates that ***huarmi yoco*** is the "strongest" kind. *Schultes et Cabrera sine num.* notes that ***yoco de brujo*** has unusually large leaves.

Further studies in the field and herbaria have done little to advance our understanding of the reasons for the Indians' recognition of these named "variants." On the contrary, the problem has been complicated by the discovery that there are many more aboriginally

FIG. 1.—*Paullinia* ***(yoco).*** Drawn by the late Gordon W. Dillon, artist, Botanical Museum, Harvard University.

named kinds of ***yoco.*** The botanists who have gathered the names and have associated them with collections of ***yoco*** cannot offer distinguishing characters. The named "variants" now known number fourteen: ***yoco; blanco yoco; canaguche yoco*** ("***yoco*** of the palm *Mauritia flexuosa*"); ***huarmi-yoco; po-yoko; tigre yoco*** ("***yoco*** of the jaguar"), ***taruco yoco; totoa-yoco*** ("white ***yoco***"); ***verde yoco*** ("green ***yoco***"), ***yagé-yoco*** or ***yoco-yajé*** (" ***yoco*** of the hallucinogen *Banisteriopsis Caapi*"); ***yoco colorado; yoco-cú*** ("reddish ***yoco***"); ***yoco de brujo*** (medicine-man's ***yoco***"); ***yoco negro*** ("black ***yoco***"). Two of these names might suggest that that kind of yoco was used with products of other plants (with ***chicha de cananguche,*** a fermented drink prepared from the fruits of *Mauritia* and with ***yajé,*** a narcotic drink prepared from *Banisteriopsis*), but that does not explain the uncanny skill of the natives in distinguishing the kind of ***yoco*** from afar.

COLOMBIA: Comisariá del Putumayo, Umbriá. "Liana. ***Verde yoco.***" January-February 1931. *G. Klug 1930*—Same locality and date. "Liana. ***Blanco yoco.***" *Klug 1933*. Same locality and date. *Klug 1935*—Same locality and date. *Klug 1937*—Same locality and date. "Liana. ***Yagé yocò.***" *Klug 1946*—Same locality and date. "Vine. ***Cananguche yoco.***" *Klug 1947*—Same locality and date. "Vine. Petals white. Strongest yoco. ***Huarmi yoco.***" *Klug 1955*—Same locality and date. "Vine. ***Taruco yoco.***" *Klug 1957*—Río Putumayo, Piñuna Negra. " ***Yoco.*** Arbusto de 1.5 m." November 20, 1940. *Cuatrecasas 10708*—Mocoa. "Bark used as source of stimulant drink. Large liana." December 3-7, 1942. *Schultes et Smith 3045*—Same locality and date. "Large woody vine. Stimulant and antifebrugal plant." *Schultes et Smith 3045A*—Río Sucumbiós, between Río Putumayo and Quebrada Teteye. "***Yoco colorado.*** Extensive liana in dense forest. Sap of cambium scraped and used also as a purge before taking ***yajé*** and to expel stomach parasites." March 19, 1942. *Schultes 3426*—Departamento del Cauca, Río Caquetá, Puerto Limoń. "Large liana. Bark used to prepare stimulant. Whitish latex-like sap." February 28-29, 1942. *Schultes 3341*—"***Yoco colorado.*** Extensive vine in dense forest. Used as a stimulant and febrifuge." May 29, 1942. *Schultes 3476*—Río Sucumbiós, Conejo. April 2-5, 1942. *Schultes 3543*—Río Putumayo, Puerto Ospina. "Extensive liana; basal diameter 3 inches; sap expressed as stimulant. Bark contains a white, latex-like sap. July 6, 1942. *Schultes 4028*—Mocoa. "Large forest liana." March 1953. *Schultes et Cabrera s.n.*—Between Mocoa and Pepino. March 1953. *Schultes et Cabrera s.n.*—Mocoa. "***Yoco negro.***" March 1953. *Schultes et Cabrera s.n.* Near Mocoa. "***Tigre yoco.***" March 1953. *Schultes et Cabrera s.n.* Along road 12 km. below Mocoa. "***Yoco colorado*** ." March 1953. *Schultes et Cabrera s.n.*—Pepino. "Extensive vine in forest. ***Yoco de tigre.***" March 1953. *Schultes et Cabrera s.n.* Rió Uchupayacu. "***Yoco blanco.*** Flowers white. Large liana." *Schultes et Cabrera s.n.*—Mocoa and vicinity. "***Yoco colorado.***" March 1953. *Schultes et Cabrera s.n.*—Pepino. "Unusually large leaves. ***Yoco de brujo*** ." March 1953. *Schultes et Cabrera s.n.*—Mocoa and vicinity. "Large forest liana. ***Yoco yajé.***" March 1953. *Schultes et Cabrera s.n.* Río Guamués, San Antonio. "Tree 4 m. Secondary growth. Bark for stimulant and to allay hunger." September 6, 1963.—Mocoa, old road to Rumiyaco. Alt. 700-850 m. "Los indígenas lo toman en maceración en frio. ***Yoco.***" October 10, 1965. *Garciá-Barriga, Hashimoto et Ishikawa 18695*—Río Putumayo Remanso. "Primary forest. Bark of stem is scraped and soaked to make a beverage which is drunk by the men early mornings while making plans for the day. ***Totoa-yoko*** ('white ***yoco***') has more leche than other types of yoco, therefore is the best type." August 22, 1966. *Pinkley 380*—Buena Vista. "***Po-yoko***. This vine, growing wild in the jungle, can grow very tall and have a diameter as thick as 4-5 inches when old. Its flower was described as 'medio-blanco' (whitish) by Francisco. It has a seed which splits open and is red inside with a black seed in the middle. This is the most common form of ***yoko*** taken by the Siona. They scrape the inner wood and drink the juice that comes out. They drink one or two mouthfuls in

the early dawn to twist the ***chambira*** fibre or perform other tedious tasks." September 13, 1972. *Piaguaje 5.*

ECUADOR: Provincia Napo, Río Aguarico, Dureno. "Primary forest. Bark of stem scraped and soaked in cold water to make a beverage which is drunk by the men very early each morning. Liana. The men drink usually together in the morning while planning the day. Kofán: ***Yoko-cú*** ('reddish ***yoko***')." January 3, 1966. *Pinkley 72*—Same locality. "Cortex of stem reddish (older than vine of no. *311*). Kofán: ***yoko-cú*** ('reddish ***yoko*** ')." June 22, 1966. *Pinkley 312* [Note: no. *311* not available for study].—Same locality. "*Totoa yoco* ('white ***yoco***')." September 6, 1966. *Pinkley 428*—Río Napo, amongst Secoya Indians. "***Yoco***." November 29, 1971. *Louthian s.n.*

III.

Banisteriopsis Caapi *(Spr. ex Griseb.) Morton* in Journ. Wash. Acad. Sci. 21 (1931) 485.

Banisteriopsis (Fig. 2) is a genus of approximately 100 species of forest lianas of tropical America. Several species have been reported as the source of an hallucinogenic drink prepared by Indians in South America—*B. Caapi* and *B. inebrians* Morton. Recent taxonomic research has indicated that the second binomial is a synonym of the first.

Vague references to this drug were made in missionary writings of the late 17th Century in Peru and Ecuador, but little was known about it until the mid 19th Century. In 1858, Villavicencio wrote about the hallucinogenic drink ***ayahuasca*** in his *Geografia del Ecuador,* but he mentioned no botanical identification beyond the fact that it was a liana (Villavicencio 1858 and 1984).

The first scientific identification of the drug was done by the British plant explorer, Richard Spruce, who in 1852 had discovered that the Tukanoan tribes of the Rio Uaupés of Brazil prepared an intoxicating drink of ***caapi*** from the bark of a liana. He collected botanical material and identified it as a new species of *Banisteria—B. Caapi*—now nomenclaturally known correctly as *Banisteriopsis Caapi.* The description of the species was not published until 1858, and Spruce's account of the preparation of ***caapi*** did not appear until 1873 (Spruce 1873, 1908 and 1970).

In 1853, Spruce met with the use of ***caapi*** amongst the Guahibo Indians of the upper Orinoco of Colombia and Venezuela—Indians who "not only drink an infusion . . . but also chew the dried stem." In 1857, when he travelled and collected in the Ecuadorian Andes, he found the Záparo and other Indians taking a narcotic drink called ***ayahuasca,*** and he reported that he considered it to come from "the identical species of the Uaupés, but under a different name" (Langdon 1985).

Since this early scientific work, many specialists and amateurs have written about the malpighiaceous narcotic of the western Amazon. We now are rather certain that it is prepared basically from the bark of one species, *Banisteriopsis* Caapi (Gates 1982). Occasionally additives may be put into the brew to alter, strengthen, change or lengthen the intoxication (Spruce 1873, 1908 and 1970). Many have been reported, but two are of importance and are widely used: the rubiaceous *Psychotria viridis* R. et P. and the malpighiaceous *Diplopterys Cabrerana* (Cuatr.) Gates (formerly known as *Banisteriopsis Rusbyana* (Ndz) Mart.). Both of these plants contain tryptamines, the first known in both families, which actually do alter and intensify the effects of the hallucinogenic β-carbolines in the basic bark of *B. Caapi* (Schultes and Hofmann 1973 and 1980).

There is no doubt that Indians in the northwest Amazon can "identify" different "kinds" of ***caapi*** or ***ayahuasca*** at a distance without feeling, tasting or smelling the liana. Sundry field studies have noted this peculiarity, and there is a long list of native names

FIG. 2.—*Banisteriopsis Caapi* ***(ayahuasca)***. Drawn by the late Elmer W. Smith, artist, Botanical Museum, Harvard University.

that are presumed to designate these numerous variants. The natives maintain that they are able to use these kinds of ***caapi*** or ***yajé*** or ***ayahuasca*** to prepare drinks of different strengths, for different purposes or in connection with different ceremonies or dances or magico-religious needs, or what the partaker wishes to kill in the hunt. At least 30 "kinds" are recognized and have native epithets in the western Amazon.

This aspect of ethnobotanical studies certainly requires much more intensive and interdisciplinary field research. Are these kinds different age forms; are they due to hardly perceptible soil or other ecological factors; are they the result of growing in semi-open or secondary situations, as opposed to the dense forest; are the specimens taken from various parts of the liana; are the cultivated specimens specially selected clones with varying chemical composition and, consequently, varying physiological effects; or are they chemovars? Langdon (1985) has written: "Apparently the native populations . . . recognize many different kinds of ***caapi*** with different hallucinogenic properties; I consider these to be chemical variants. The ease with which ***caapi*** can be vegetatively propagated . . . makes it possible for clones of such variants to be maintained." This statement might explain a limited few cases of cultivated plants, but it cannot satisfactorily be considered an explanation of the problem for wild lianas, nor can it in any way clear up the Indian's ability to identify these variants ocularly from a distance. Langdon (1985) further states that the western Siona of the Putumayo of Colombia, Tukanoan-speaking peoples, with whom she worked, use "finer distinctions than the botanist in classifying plants. One feature employed in their classification is botanical: length, breadth, size and leaf pattern and whether or not the plant flowers; another feature concerns the part of the plant used; another refers to phases of growth; still others are distinguished . . . on the kind and colours of the visions induced; the strength of the intoxication also enters as a factor. Other differences are taken into account: each plant has a spiritual guardian and a shaman owner, and shamans often trade kinds. Furthermore, if a shaman finds a wild liana in the forest, he will prepare a drink to ascertain its worth for inclusion in his own repertoire, especially in regard to what visions it can induce; if he takes a cutting, he will then and there name and classify it."

It is difficult for the scientist to understand or accept many of these "criteria", as real as they may be to the Indian, but native perspicacity in the finer classification of many plants—both wild and cultivated—should be critically examined for the possible practical values of some of the points of evaluation to taxonomists and phytochemists.

On the basis of extensive field work in Peru, Rivier and Lindgren report that the Sharanahua Indians distinguish three types—red, black and white "kinds", and that the distinction is based more in the differentiation in colour of the drink than in the appearance of the plant (Rivier and Lindgren 1972)

According to another field investigator, Deltgen, there are in the Colombian Vaupés six kinds of caapi, based primarily on their effects (Deltgen 1978-79).

Still another specialist, Reichel-Dolmatoff, states that there is in the Vaupés "a large series of kinds" distinguished mainly on the basis of psychoactive effects (Reichel-Dolmatoff 1975).

The most comprehensive field investigation of the Indian recognition of "kinds" of the narcotic liana is that of anthropologist Langdon amongst the western group of Tukanoans—the Siona—who live in the Colombian Comisaría del Putumayo, far to the west of the Tukanoan tribes of the Vaupés. Langdon was able to collect 18 different vernacularly named "kinds". Botanical material of almost all was collected and identified by Dr. Timothy Plowman of the Field Museum of Natural History in Chicago (Langdon 1985). Almost all are botanically referrable to a single species: *Banisteriopsis Caapi*.

Siona classification "is seen", according to Langdon, "as more complex than that of botany and depends on the conjunction of botanical features, chemical effects of the

mode of preparation and cultural suggestions in the visions experienced" (Langdon 1985). It is undoubtedly true that all of these criteria are employed by the Indians for eventual "classification" of the kinds of *Banisteriopsis Caapi*, but it is still not possible for a native in the forest ocularly to identify with certainly a kind of caapi by vernacular name from such features as its chemical constituents and culturally—what he believes that the drug may induce.

The meticulous field research of Langdon has indeed produced a good start, but, in her own words, "further exploration between this conjunction of botany-chemistry-culture warrents further investigation". It is still an enigma.

COLOMBIA: Comisaría del Putumayo, Río Uchupayaco, southeast of Puerto Limón. "***Yajé***, Narcotic. Liana." February 27-28, 1942. *Schultes 3346*—Río Sucumbíos, Conejo, March 29, 1942. *Schultes 3475*—Mocoa, August 28, 1963. *Juajibioy C. 256a*—Mocoa. "*Bichemia* ('bejuco'), ***amarrón huasca*** ('vine of the boa')." August 28, 1963, Juajibioy C. 279—Mocoa. "***Inde huasca***." August 28, 1963. *Juajibioy C. 280*—San Antonio del Guamués. "***Yajé del monte***". August-September 1963. *Naranjo et Wiederhold 4*—San Antonio del Guamués. "***Yajé sembrado***." August, September. *Naranjo et Wiederhold 5*—Buena Vista. "Celima's ***wai yagé***. Celimo grows it in his field. It is a tall and thick vine. It is cooked before drinking, with ***yagé-oko*** added. Francisco Piaquaje said that it is another class, but he can't identify it. Luciano Piaquaje recognized it as ***wai' yagé***." September 1972. *Langdon 21*—Buena Vista, "***Beji yage***. This ***yagé*** is cultivated in the fields. It is tall and very thick at full growth. It is cooked with ***yagé-oko*** and is supposed to be one of the strongest ***yagés***" September 26, 1972. *Langdon 23*—Buena Vista "***Wa'i yage*** or ***wahi-yagé*** collected and grown by Francisco Piaquaje. The ***yagé*** is a ***mata*** (bush) instead of a vine. It is about 1 meter tall. It is grown in their fields. The plant grows as a thick bush. This ***yagé*** is usually prepared simply by grating the stems and drunk without cooking. It is used to see the ***pinta de caseria***—visions of animals. The leaves may possibly be mashed and their juice used also." September 26, 1972. *Piaquaje 25*—Buena Vista. "***So'om-wa-wa'i yagé***. Collected and grown by Ricardo Yaiguaje. This ***yage***, which Richard grows in his field, is given its name because of the way it grows. The vine, which is about 3-4" thick at maturity, grows very, very long: ***so'-om-wa***. To make it grow thick, Ricardo prunes the branches. This is a class of ***wahi-yagé*** and they drink it raw by grating the vine. However, they also mix it with various classes of ***kwa'ku-yagé***. It shows the visions of hunting. Leonides, Ricardo's father, told him to plant only this in a field, and the ***yagé*** would be like the seed of the wild pig ***sese*** so that even though the Siona no longer drink ***yagé***, they would always have a lot of game. I asked him if this was ***sese yagé***, and he said: 'Lo mismo'." September 27, 1972. *Langdon 30*—Buena Vista "***Usebo yagé***—collected by Ricardo Yaiguaje. The vine of this class of *yage* is very thick, about 6" in diameter. The vine grows straight up instead of twisting, until its thickness begins to diminish. This is one of the ***kwa'ku*** class and is cooked with ***yagé-oko***." September 27, 1972. *Langdon 31*—Buena Vista. "***Wa'i yagé***. It is a bush about 1-1½ meters high." September 28, 1972. *Langdon 32*—Buena Vista. "***Weki-yagé***. Grown by Celimo Amo. It is a large vine. Is a class of ***kwa'-ku yagé***." September 29, 1972. *Langdon s.n.*--Between Mocoa and Pepino. "Stout vine on trees, 45 ft. tall. Leaves membranaceous, dark green, shining above, pale green beneath. Stems boiled with ***chagropanga*** *(Diplopterys Cabrerana)* to make narcotic drink. ***Indehuasca. Yajé***." July 28, 1960. *Schultes 22553*.

INTENDENCIA DEL GUAINIA: Río Inirida, El Remanso. August 12, 1975. *García-Barriga 20805*.

ECUADOR: Provincia Pastaza, Río Chico, affluent of Río Pastaza, Village of Río Chico. Alt. c. 1000 m. "Shredded stem boiled with fine leaves of *Diplopteris Cabrerana (Shemluck et Ness 218)* until conc. and ca. 3 oz. taken on empty stomach for hallucinations. The day fast not absolute; bananas and salt can be eaten. *Ayahuasca.*" August 1979. *Shemluck et Ness 219.*

PERU: Departamento de Loreto, Iquitos. "***Ayahuasca***. Woody vine. A tea brewed from the leaves and stem produces fanciful dreams. Also used as a cure for many ills and as an intoxicating beverage. A strong narcotic." August 2-8, 1929, *Killip et Smith 27385*—Iquitos region, Río Nanay, Picuruyacu "***Ayahuasca***. Vine growing near garden. Chopped vine boiled with ***yajé*** and ***samiruca*** *[Psychotria viridis].*" July 5, 1966, *Martin et Lau-Cam 1089.* Río Amazonas. "Trepadora sin flores ni frutos. ***Haya huasca***." October 25, 1966. *Torres 223.* Zapote, Alto Río Purús. "Lowland forest. The stems are mashed and boiled with water during one hour with one or more additives to prepare ***ayahuasca***." August 22, 1968. *Rivier 1*—Zapote, Alto Río Purús. "Lowland forest. ***Ramiwetsem*** ('yellow *ayahuasca*'). Culiba name" August 22, 1968. *Rivier 2.* Marcos, Alto Río Purús. "The stems are mashed and boiled in water for about one hour with one or more additives to prepare ***ayahuasca***. Lowland forest." August 8, 1968, M68. *Rivier 3.* Marcos, Alto Río Purús. ***Shurioshinipa*** ('red ***ayahuasca***') = name in Sharanahua. August 8, 1968. *Rivier 4.* Rio Amazonas, Caballacocha. "***Ayahuasca***. El remedio. Extensive woody liana 6 ft. high with trunks to 10 cm. diameter. Cultivated in chacra near lake. Stems chopped up and boiled for 8 hours to prepare hallucinogenic beverage, mixed with ***chacruna*** leaves *(Psychotria vividis).*" March 22, 1977. *Plowman, Schultes et Tovar 6430.* Departamento de San Martín, Huahuiva. "***Ayahuasca negra***." July 6, 1985. *Woytkowski 5045;* 5074.—"Monte real. ***Shillinto o ayahausca amarilla***. Medicinal." July 10, 1958. *Woytkowski 5076*—San Alejandro, Río de Loreto. "Monte real. 300 m. ***Ayahuasca amarilla***." July 24, 1958. *Woytkowski 5119*—San José de Sisa, c. 550 m. "Trepador voluble (soga) cultivado. Flores rosadas. ***Shimba-ayahuasca***." July 26, 1958. *Velarde N. 6577*—Tarapoto. Río Schilcayo. Alt. 350 M. "Extensive cultivated liana, 6 in. tall, growing in full sun. Sterile. ***Ayahuasca***." May 4, 1976. *Plowman 6041.*

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The Cactus Primer. A.C. Gibson and P.S. Nobel. Pp. vi + 286; illustr.; Harvard University Press, Cambridge, Mass. 1986. $39.95.

As the authors state in their preface: "People around the world and from all walks of life are hopelessly susceptible to a condition called cactophily, the love of cacti." This interest has led to a plethora of books on the cactus family— mostly dedicated to classification or horticulturally curious specimens and their care as house plants. Here we have a different book—one that, to my knowledge, is the first to present in a single volume such a wealth of data on the biology and structure of this most misunderstood family of xerophytes.

The topics discussed span a broad spectrum: general features; early evolutionary trends; special features; chemistry; how structure and chemistry can help unravel phylogeny; and, finally, relationships of the family. The extensive glossary is a very helpful addition; the detailed index unlocks with ease much of the information in the volume. Each chapter has its own bibliography.

As a contribution of true biological value, this volume will be welcomed by botanists, horticulturists and amateur cactophiles alike, and especially by those interested in the drier parts of the world, for it is not possible to find such a mass of scattered information in one book—and amassed and expertly evaluated by two recognized specialists in the group.

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ABORIGINAL EXPLOITATION OF PRONGHORN IN THE GREAT BASIN

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ABSTRACT.—Ethnographic, historic, and archaeological data concerning pronghorn exploitation in the Great Basin are presented in a framework that allows for a revision of prevailing models concerning this activity. It is proposed that various ethnographic and historic accounts do not accurately reflect the prehistoric contexts of pronghorn exploitation. Exception is taken with the Pendleton and Thomas (1983) model, which proposes that the use of permanent, labor-intensive drive structures diminishes through time in the Great Basin.

INTRODUCTION

The American pronghorn *(Antilocapra americana)*, distinctive in both its evolution and behavior, was an important source of food and clothing for Indian peoples of central and western North America. Many ethnographic and historic sources mention both the ceremonial preparation for, and the actual hunting of pronghorn; and pronghorn remains are present in the faunal assemblages of numerous Great Basin sites. In spite of this, archaeological data concerning the exploitation of pronghorn by aboriginal groups in the Great Basin has not been synthesized with ethnographic data on the pronghorn. A revision of several prevailing concepts concerning pronghorn hunting that appear in the literature, and which have apparently been accepted by many North American anthropologists in lieu of a critical examination of the available data, is suggested. Various models concerning this important economic and social activity appear to be based largely on both post-contact- and negative data, and probably are not applicable to the pre-contact practice of pronghorn exploitation.

EVOLUTION AND ETHOLOGY

Based on the geological contexts of fossil specimens, it has been estimated that several genera of antilocaprids have existed in North America since the middle Eocene (ca. 17-70 million years B.P.), and apparently their ranges were restricted to west of the Mississippi River in post-mid Pliocene times (Einarsen 1948:1-2; Webb 1973:203). The pronghorn is the only living representative of the bovid subfamily Antilocapridae (Kitchen and O'Gara 1982:960), as pre-European pronghorn populations are estimated at 30-40 million individuals (Cahalane 1947:69; Yoakum 1978:103), and are believed to have occupied a large portion of western North America (Fig. 1). Fossil evidence suggests that many genera were present during the Pleistocene (Yoakum 1978:103; Kitchen and O'Gara 1982:960), and that the present population is the sole survivor of one genus.

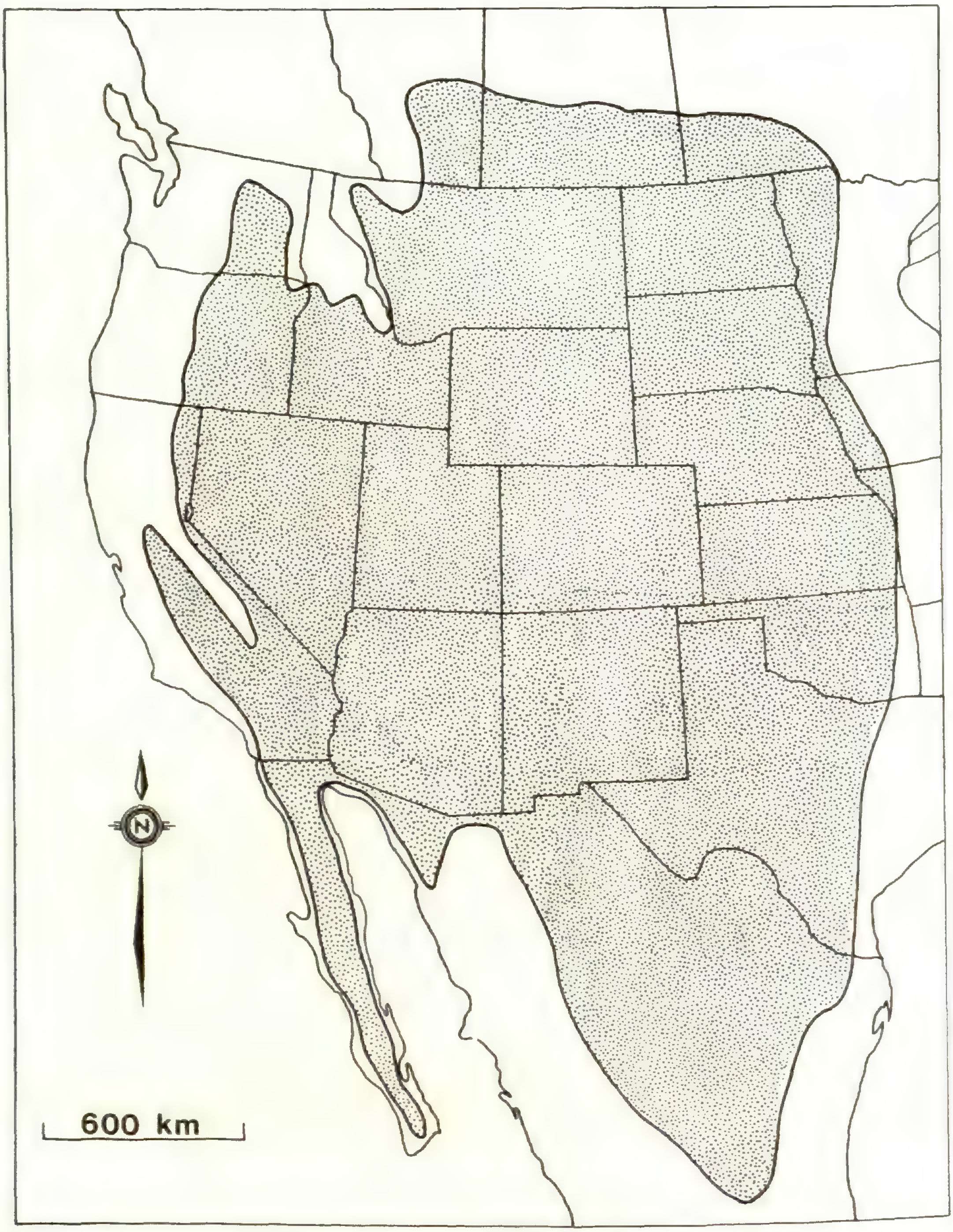

FIG. 1—Geographic distribution of post mid-Pliocene *Antilocapra americana*. (Taken from S. D. Webb, 1973)

The behavior of pronghorns is a central issue here, as several predictable reactions were effectively utilized by aboriginal hunters in the capture strategies that were applied to this animal. In general, pronghorn do not follow any set pattern of migration, as do other animals such as the caribou *(Rangifer tarandus)*. Einarsen (1948:11-12) stated that " . . . [pronghorn] may change feeding grounds several times within the year, but their drift from one range to another is not usually a long trek, lacks rhythm, and will often be northward as southward in winter". This means that they could not be hunted by planning where they would be at any point in time, but instead could be profitably hunted by opportunistic groups, usually during the fall and winter, when they come together in large herds. From southeastern Oregon, Einarsen (1948:41-42) reported that "On Drake's Flat in [the winter of] 1941, a band of nearly 1,000 animals was using only about 10 sections of land."

Gregarious by nature, pronghorn were ideal targets for large scale hunts, a hunting strategy possible, but not necessarily always practiced, throughout the Great Basin as well as portions of central and eastern California. Kroeber (1925:528) stated that Yokuts in the San Joaquin Valley of California hunted herds of pronghorn that numbered in the thousands. It has been reported that communal drives reduced local pronghorn populations to such an extent that at least several years were required to restore the herds to a sufficient size before a large drive could be held (Egan 1917; Steward 1938; Shimkin 1947). It seems that the concept of large scale hunts has been taken from one or two accounts of post-contact pronghorn drives among the Gosiute Shoshone (Egan 1917:240) and applied to the entire Great Basin. A principal objective of this paper is to present evidence supporting the view that pronghorn were plentiful in the Great Basin in pre-contact times and hunted in all seasons, in both individual and communal contexts (Murphy and Murphy 1960:308; Kelly 1964:50).

Another behavioral trait of pronghorns which helped hunters to more easily drive and corner them is that when confronted by fences and walls, instead of leaping over them, they prefer to either crawl under or through them, or to run along the barrier until it ends. This characteristic was an important factor in the success of Indian peoples when driving herds into wing traps and corrals, as they did not have to construct tall barriers or utilize exceptionally strong materials for such devices.

Although extremely fast, pronghorn have a habit of running at top speed until they are out of sight of whatever scared them, but then seem to forget about what they were fleeing from, and quickly settle down to graze. Consequently, a single hunter could successfully stalk them (Frison 1978:252).

ABORIGINAL HUNTING METHODS

Ethnographic literature indicates that all Numic-speaking groups of the Great Basin exploited the pronghorn often and in various ways. Kelly (1932:83) reported that the Surprise Valley Paiute held communal drives in fall as well as winter. Another season is added to this pattern by Lowie (1924:304), who stated that "Early in the spring, when the ground was muddy and someone had seen a big [pronghorn] herd, the Indians [Northern Paiute] would gather for the communal chase." Steward (1941:220) reported that various Nevada Shoshone peoples hunted pronghorn in both spring and fall. Furthermore, Murphy and Murphy (1960:308) stated that several Northern Shoshone groups, such as the Bannock and Lemhi, would pursue pronghorn whenever possible on an individual basis, in addition to large, formally planned drives in fall and winter. In her ethnography of the Southern Paiute, Kelly (1964:50) maintained that in the area occupied by the Kaibab, pronghorn could be hunted "any season of year".

In addition to occupying the plains and grasslands of the Great Basin, pronghorn also ranged throughout the eastern Sierran foothills, as they were hunted by the Washo (Curtis 1926:171-172), Mono Basin Paiute (Curtis 1926:167), Owens Valley Paiute (Steward 1933:253), Panamint Shoshone (Steward 1938:72, 233), Kitanemuk (Harrington 1942:6), and Kawaiisu (Vogelin 1938:13).

Shamanism.—Pronghorn hunting methods of historic Great Basin groups were considerably more elaborate than those employed for deer *(Odocoileus* spp.). The majority of Numic-speaking peoples relied on pronghorn shamans to charm the animals, capture their souls, and lure them into corrals made of wood (usually juniper) and sagebrush (Steward 1941). Pronghorn "charming" was widespread among the Northern Paiute (Lowie 1924:302-303; Stewart 1941:423; Riddell 1960a:40), Nevada Shoshone (Steward 1941:219), and some Northern Shoshone bands (Steward 1938:34).

Only a minority of Great Basin groups hunted pronghorn without the aid of a shaman. Some of the mounted Shoshone bands, such as the Bannock, Snake River, and Lemhi, did not practice charming, because they did not use corrals (Murphy and Murphy 1960:308). Instead, they secured pronghorn by surrounding herds on horseback and running them in circular relays until the animals dropped from exhaustion (Steward 1943:266). Kelly (1964:50) reported that most Southern Paiute groups did not rely on charming, as they also did not use corrals. The use of shamans seems to have occurred among groups that practiced large communal drives via corrals, and who did not use horses extensively. These ceremonial specialists acted as psychological aids for pronghorn drives, as they supposedly disoriented the animals, and caused them to enter the corrals. The charming of pronghorn required from three to five days, depending upon the particular group, and involved dancing, smoking, chanting, singing, and symbolic slaying of pronghorn (Hopkins 1883:55-57).

Individual hunting.—A lone hunter equipped with bow and arrow would often wear a pronghorn skin and head in order to approach a herd of pronghorn. The head would be that of a male, so the hunter could portray a lone buck approaching the herd. This practice occurred in all major areas of the Great Basin, being utilized by various Northern Paiute (Lowie 1924:197), Gosiute (Steward 1943:294; 1941:220), Ute (Kelly 1932:82-83; Stewart 1942:241), and Southern Paiute bands (Kelly 1964:50). In her 1932 ethnography of the Surprise Valley (northeastern California) Paiute, Kelly (1932:82-83) provided an excellent description of the way in which this animal was hunted individually.

> The hunter wore an antelope head with horns attached, and also a hide body disguise. The latter was not necessary for deer stalking, but was required for the more alert antelope. The hunter carried white paint in a sack, and when he approached the animals he smeared it on his face and arms and on his body and legs below the hide covering. He carried a stick about the length of the antelope's leg as an aid in walking and gradually got within shooting range by imitating the movements of the animal, pawing the ground and simulating grazing in order to make the deception realistic.
>
> Other techniques of individual pronghorn hunting involved hiding in ditches and behind shrubs next to springs and game trails, from where a marksman could effectively ambush these animals (Steward 1941:272; 1943:360).

Mounted hunting.—A method of taking pronghorn that is intermediate between individual and communal strategies in terms of the number of people required was to use horses in relays to run down and tire the animals. This method was mostly used

by Northern Shoshone groups which had adopted the horse. This was obviously a late development, and usually required between six and twelve riders to encircle the pronghorn and run them. This actually was the least productive mode of pronghorn hunting, as numerous horsemen might work a small herd all day and catch only a few animals. In his ethnography on the Northern Shoshone, Lowie (1909:185) observed that "It would sometimes take forty or fifty hunters half a day to kill two or three antelopes by this method."

Although not the most effective technique, running down the speedy pronghorn on horseback was a popular activity for certain Great Basin-Plateau peoples, as the Bannock, Snake River, Wind River, and Lemhi Shoshone all practiced this technique in historic times (Shimkin 1947:268; Murphy and Murphy 1960:308-310).

Surround method.—Formal groups would sometimes gather to surround a large herd of pronghorn, and many could be caught by participants gradually closing the human circle in on the quarry (Vogelin 1938:13; Shimkin 1947:268). Dogs were also used to help drive pronghorn towards archers hidden among rock outcrops or shrubs (Lowie 1909:185; Steward 1941:367).

Drive fences and corrals.—Drive fences and V-wings seem to have been the constructions most often used to secure pronghorn. They may also represent forms that preceded the corral, as they are of a simpler design and allow some animals to escape. It is possible that the corral was incorporated into the V-wing and drive fence at a later time.

The most basic drive fences were either rock or sagebrush alignments with a gap in the center. Drivers on foot would herd the pronghorn in the general direction of the fence, where archers would be hiding in blinds along the passageway. The animals would usually head for the opening, and could be shot as they passed through (Kelly 1964:50).

Some Southern Paiute groups evidently constructed parallel rock alighments which formed a type of gauntlet, with rock blinds being staggered on both sides every 60 m or so (Kelly 1943:32). The animals were driven through these channels and shot by hunters stationed in the blinds.

V-wings were implemented by practically all Numic-speaking peoples; even those with horses are reported to have built them. These traps could be erected by simply pulling up sagebrush in the appropriate formation, after which the quarry was driven between the arms, towards hunters concealed near the bottleneck (Chamberlin 1911:335-336). The arms of these V-wings were usually separated by a great distance at the mouth, as this ranged anywhere from one to ten km, and would gradually funnel down from there. Many bands held drives in early spring, when the ground was muddy and footing poor for the pronghorn (Lowie 1924:304). It was also common for the sagebrush wings to be set afire, presumably increasing confusion and preventing the animals from doubling back (Lowie 1924:304; Steward 1941:219; Stewart 1941:367).

Corrals are reported to have been widely used, and this may be because this method was a relatively late development, and therefore, still fresh in the minds of many informants. The corral was certainly a superior method in terms of maximizing results, for if everybody did his job correctly, an entire herd could be trapped.

Corral traps were usually constructed with long drive fences or V-wings leading to them (Fig. 2), and tended to be placed in areas where there was either a rolling and/or shrubby topography (Egan 1917:239), as this served to hide the corral from a distance.

The use of corral traps has been reported throughout the Great Basin. These structures were usually considered to be the property of the group or groups that built them, as they were often maintained and repaired from year to year (Steward 1938:175). This would seem to indicate that in the recent past, annual drives were held by various Great Basin groups. Corrals were often built by establishing a wide ring of juniper posts

and rocks, with sagebrush being incorporated to give the barrier a solid appearance (Fig. 3). Adequate corrals could also be constructed by joining together strands of sagebrush bark rope and arranging the rope in a circle, hanging it from sagebrush (Stewart 1941:422; Riddell 960a:40).

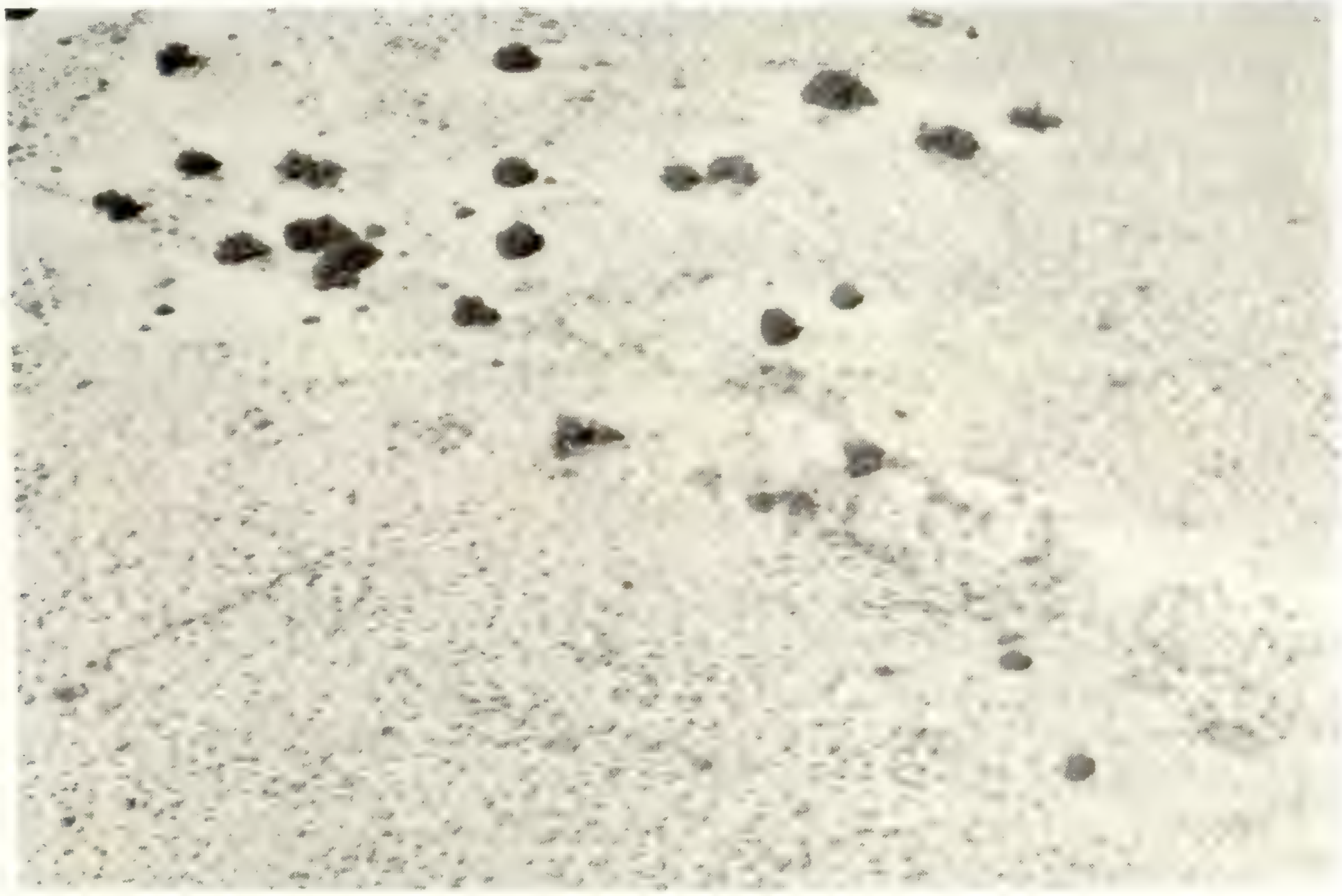

FIG. 2—Aerial view of historic V-wing trap (#3) in Mono Basin, California. Live juniper trees are incorporated into west wing (which runs for 600 meters) at top center, east wing is located at left foreground, and corral is situated in mouth of seasonal wash, at right middle. Photo: Eric J. Levy

Egan (1917) recorded an account of a pronghorn drive in which he participated in the late-1800s with a Deep Creek Gosiute band. This narrative is highly informative, and provides a vivid picture of what these drives were like.

> For a few days before I came the squaws and bucks were busy repairing and extending the flanking arms of the old corral, or trap pen, which was located near the north end of antelope valley and about twenty miles northwest of Deep Creek . . . After they had all come in from their work a great deal of talking and planning was on and each knew just what part and place he or she was to take . . . They were to spread apart across the valley, travel in open order back to the north, being careful that not one of the antelope jumped would run, except in a northerly direction . . . The arms or leads are started at the extreme ends by simply prying or pulling up a large sagebrush and standing its roots up on the top of another brush, thus making a tall, black object visible for miles . . . There were many turns to the lane thus formed, but were getting narrower and stronger till finally, around a sharp turn through a large, thick bunch of cedars, the game were in the corral, which was about two hundred feet in diameter . . . The pine and cedar trees had not been removed

FIG. 3—Aerial view of historic corral (trap #3). The corral entrance is located at top right, between two live juniper trees. Corral perimeter is 145 meters. Photo: Eric J. Levy

> from the inside of the pen, and not many from the runway, for a mile back [1917:239-240].

ARCHAEOLOGICAL EVIDENCE OF PRONGHORN PROCUREMENT

A review of the literature concerning archaeological sites in the Great Basin and neighboring areas indicates that pronghorn have been exploited by aboriginal groups since early occupation of western North America; as this practice spans the Paleoindian period to historic times. In many areas, it appears that this animal provided a substantial portion of the meat consumed by inhabitants of seasonal camps and villages.

Terminal Pleistocene examples.—Although these data are derived primarily from the Plains, they are important to this discussion, as these late Pleistocene/early Holocene kill sites give some indication of the general time depth involved in pronghorn exploitation in western North America. Three sites in eastern Wyoming that are representative of Clovis, Folsom, and Agate Basin traditions have all yielded pronghorn remains.

The Agate Basin site was a seasonal camp at which bison *(Bison bison antiquus)* or *(Bison bison occidentalis)* procurement and utilization was emphasized (Frison and Stanford 1982:2). The site has a stratigraphic record which covers slightly over 10,000 years of occupation, based upon a sequence of eighteen radiocarbon dates which yielded early and terminal occupations of 11,840 ^{14}C years B.P., and 1520 ^{14}C years B.P., respectively, with at least six pronghorn represented in the Clovis, Folsom, and Agate Basin components (Frison and Stanford 1982:179). The investigators concluded that these particular animals probably were killed away from the site through individual efforts (Walker 1982:270, 273).

The Casper site, also a temporary camp thought to have been occupied during late fall, contained diagnostic pronghorn bone from a Hell Gap context (Frison 1974:108). This material is seen as representing a terminal stage of the Agate Basin complex, with the most intensive use of the site occurring approximately 10,000 years ago (Frison 1974:108).

The Sister's Hill site is unique to the prehistory of the Plains, as bison remains were not recovered from this single component Hell Gap expression (Agogino and Galloway 1965:191-192). The charred bones of pronghorn, mule deer *(Odocoileus hemionus)*, porcupine *(Erethizon dorsatum)*, ground squirrel *(Citellus* spp.), and other small rodents comprised the faunal assemblage of the Sister's Hill site (Agogino and Galloway 1965:192). This site was occupied during early Holocene times, as a single radiocarbon determination of of 9650 ^{14}C years B.P. was derived from a composite charcoal sample (Agogino and Galloway 1965:192).

Early Holocene record.—Many sites in the Great Basin have yielded pronghorn remains, indicating that this animal has been an integral part of human economies since the earliest clearly recognizable occupation of the region. Pronghorn bone was found in all cultural horizons at both Danger Cave and Juke Box Cave, with major occupations of these sites falling between 10,400 ^{14}C years B.P. and 4000 ^{14}C years B.P. (Jennings 1957:93). From the faunal assemblages of both sites, it was determined that the main game resource had been the pronghorn (Jennings 1957:224). Unfortunately, the minimum number of individuals (MNI) was not calculated, as only the total number of diagnostic ungulate bones (2337) appeared in text (Jennings 1957:224).

Dirty Shame Rockshelter, located in extreme southeastern Oregon, is another site the provides a lengthy occupational record. This rockshelter has been a focus of human activity for at least 9000 years, as early and late radiocarbon dates of 9500 ^{14}C years B.P. and 365 ^{14}C years B.P. have been obtained (Aikens, Cole, and Stuckenrath 1977:6-7). Of the six distinct cultural zones at Dirty Shame Rockshelter, five of them contained pronghorn remains, representing at least thirteen individuals (Grayson 1977:6, 11).

Middle and late Holocene period.—No less than eight sites dating from middle Holocene times, and distributed throughout the western Great Basin, have yielded varying amounts of pronghorn bone. These include Gatecliff Shelter (Thomas 1983b), Silent Snake Springs (Layton and Thomas 1979), Lovelock Cave (Loud and Harrington 1929), Southfork Shelter (Heizer, Baumhoff, and Clewlow 1968), Hidden Cave (Thomas 1985), the Karlo site (Riddell 1960b), and the Rose Spring site (Lanning 1963).

The occupations of all these sites occurred between 5800 ^{14}C years B.P. (Thomas 1983b:42-43) and 145 ^{14}C years B.P. (Heizer and Napton 1970:3, 39). MNI calculations varied from site to site, but generally fell between three (Lanning 1963:287) and eleven animals (Thomas 1985:138-139). It should be stressed that at all of these sites, much of the mammal bone was highly fragmented, possibly due to marrow extraction and tool production activities, and not identifiable at the genus and species level. Because of this, there is the distinct possibility that a large percentage of this material may have been that of pronghorn.

Various Fremont sites, located in the central Great Basin, contained butchered pronghorn bone. These include Median Village (Marwitt 1970), Caldwell Village (Ambler 1966), Bear River No. 2 (Aikens 1967), and Bear River No. 3 (Shields and Dalley 1978). Occupation of these sites ranged from 1450 ^{14}C years B.P. (Shields and Dalley 1978:63) to 995 ^{14}C years B.P. (Aikens 1967:35). At both the Bear River No. 2 and Bear River No. 3 sites, bison *(Bison bison bison)* was the most plentiful game animal, with pronghorn and mule deer being the next most important source of mammal meat (Aikens 1967:55, 57; Shields and Dalley 1978:94). At Caldwell Village, 125 pronghorn bones were

identified, as this mammal was by far the most important game animal, estimated to have supplied 65% of the meat consumed there (Ambler 1966:105). Cultigens were probably the primary food source for the inhabitants of Median Village (Dalley 1970:9), but at least four pronghorn were represented in the food remains recovered there (Dalley 1970:129).

Wooden traps and rock walls.—Game drive structures of the Great Basin have traditionally received little attention from archaeologists. However, recent work indicates that communal game drives were an important facet of Great Basin subsistence strategies, as the use of these features agrees with the general economic and demographic models presented in the ethnographic literature.

The Eden-Farson site is a proto-historic camp located in the Green River Basin of Wyoming. Remains of over 200 pronghorn were recovered from 12 house structures, indicating some sort of communal trapping operation (Frison 1971:258). Evidence suggests that this site was occupied only once, sometime during October and November, based upon the eruption of the premolars of pronghorn specimens (Frison 1971:258; Nimmo 1971:287-288). A single radiocarbon date of 230 ^{14}C years B.P. further suggests a late prehistoric context (Frison 1971:258).

Although the remains of a drive structure were not found, a logical explanation for such a large number of animals being taken within such a short time is that some type of communal trap was used (Frison 1971:266).

The Little Whisky Flat site (NV-Mi-5) is located at the base of the Excelsior Mountains, near Walker Lake, in westcentral Nevada. The game trap there was first recorded by Edward Kern, a member of the 1845 Fremont expedition, which traveled across the Rocky Mountains and into California. In his diary entry of December 11, 1845, he mentions passing by "A large corral or pen made of sage and cedars for the purpose of ensnaring deer." (Kern 1876:481).

Kern was apparently incorrect in assigning to this corral the function of trapping deer, as a recent (1984) archaeological investigation of this site resulted in the recovery of a large amount of butchered bone from several house structures located on a hill overlooking the corral. Within the faunal assemblage, over 90% of all diagnostic specimens were those of pronghorn, with fish, jackrabbit *(Lepus californicus)*, and rodents representing the other 10% (R. M. Yohe II, personal communication 1986).

Subsequent field surveys resulted in the discovery of a single-wing leading to the corral. Local juniper *(Juniperus occidentalis)* was used for much of the material, and two "modern" ($<$100 ^{14}C years B.P.) radiocarbon dates (UCR-1639; UCR-1640) assayed on carbon samples collected from the outer rings of corral and wing posts indicate a late prehistoric/proto-historic use of the site, as it was probably occupied by a segment of Walker Lake Paiute (P. J. Wilke, personal communication 1985).

The Mono Basin Winter Village site (CA-Mno-2122) is located approximately 30 km southwest of the Little Whisky Flat site. This is an ethnographically known proto-historic Kuzedika Paiute village (Davis 1965) located in eastcentral California. The site was examined (and formally recorded) by the author during the summer of 1985. A complex of three V-wing corrals and at least ten house structures were recorded. All three traps were made of local juniper, with two having been burned to ground level (most likely by range fires), and one being fire scarred, but almost completely intact (Figs. 2 and 3).

The two burned traps are believed to have been constructed shortly before extensive contact with European populations (A.D. 1850), as the juniper posts were most likely felled by fire, and show no evidence of nails or wire having been used in construction or upkeep. Another indication of a relatively late construction date for traps #1 and #2 is the occurrence of Desert Side-notched and Cottonwood Triangular projectile points

within several activity loci directly associated with these drive structures. A map of corral #2 is presented to supply the reader with a general idea of the basic plan of a V-wing trap (Fig. 4). All three drive structures have been incorporated into consecutive washes, as these natural channels no doubt aided in funneling the quarry toward the corrals.

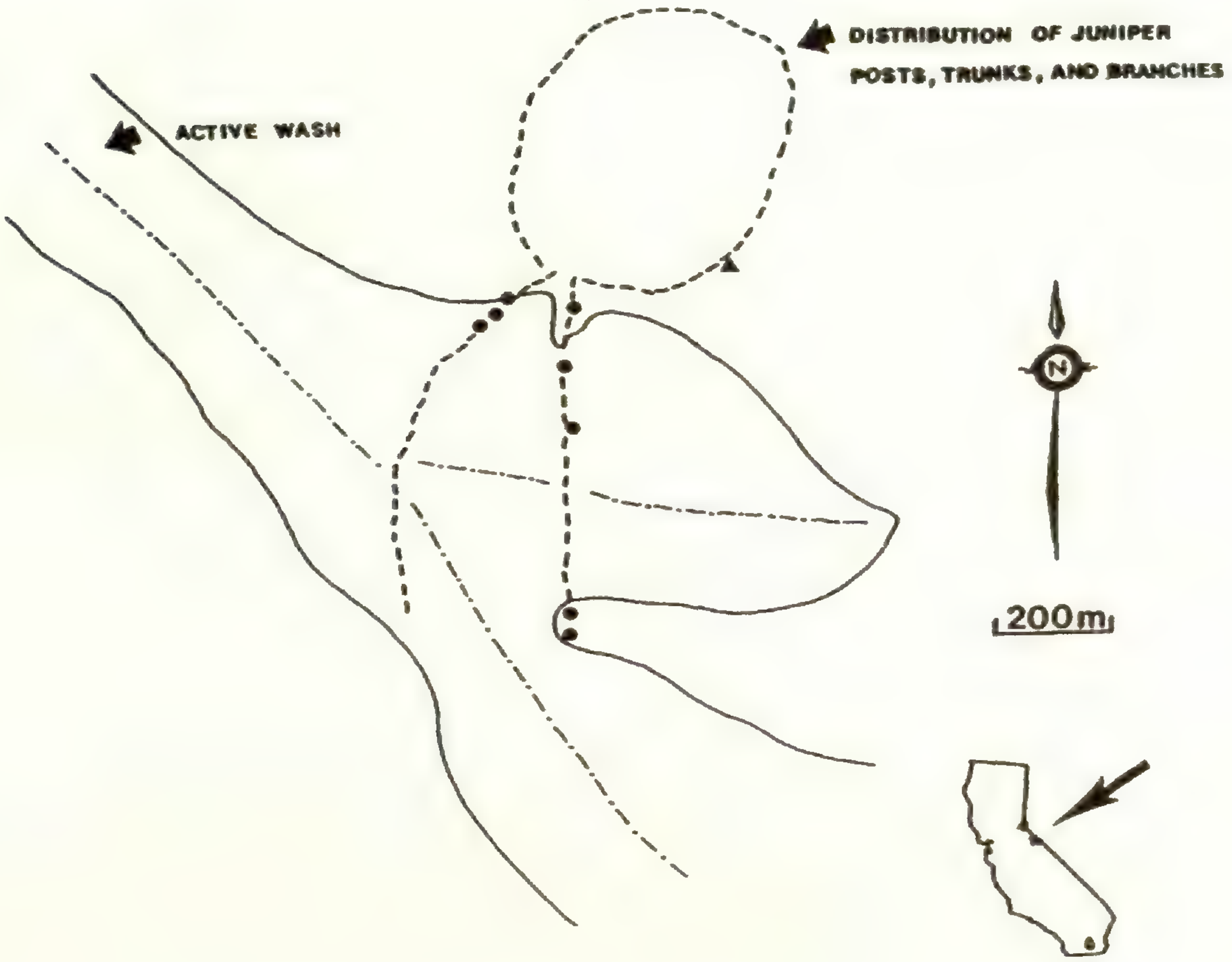

FIG. 4—Map of pre-contact pronghorn trap (#2) in Mono Basin, California. Dots represent live juniper trees, and triangles represent activity loci.

Two diagnostic pronghorn bones were found in surface association with the east and west wings of traps #1 and #2, respectively. Both specimens are astragali (R. M. Yohe, personal communication 1985); with one being fire-affected, and exhibiting what may be a butcher (cut) mark on the lateral anterior portion (J. D. Kent, personal communication 1986).

Two "modern" radiocarbon dates (<150 ^{14}C years B.P.) (UCR-2096; UCR-2098) have been assayed on two carbon samples collected from the outer rings of the proximal portions of fence posts from both pre-contact corrals. These determinations indicate that the traps at this site, as well as the one at Little Whisky Flat, are younger than approximately 350 ^{14}C years B.P., as no greater precision is possible because of short term variation over the last few hundred years in atmospheric ^{14}C activity.

From the available data, it appears that two of the drive structures probably were used for communal pronghorn drives. The function of the standing corral is more uncertain, as it was originally built sometime during the late 1800s. Over 90% of the posts of the standing wing trap were felled by axe and saw, and various sections were

reinforced with square- and round-cut nails and bailing wire. There is a distinct possibility that trap #3 was used for capturing feral horses (P. J. Wilke, personal communication 1985), as the pronghorn population of the Mono Basin may have been drastically reduced by the influx of settlers in the mid-to-late 1800s, and it was no longer possible nor practical to attempt a drive. Surface faunal remains have not been found associated with the historic trap, and further investigation is needed to determine the function of this structure.

Two other examples of aboriginal corrals that have been recorded in recent years are located in Box Elder County, Utah (Raymond 1982:29). Both elliptical enclosures are composed of juniper trunks and branches, with most of the fencing having tumbled over, but lying in a recognizable formation (Raymond 1982:26-27). Reconnaissance around both structures failed to yield evidence of aboriginal occupation within the immediate vicinity, but an open juniper grove 100 m east of one corral contains many old and weathered cut stumps that may have been the source of wood for trap construction (Raymond 1982:28). It has been determined that both structures were built in post-contact times, owing to the fact that various posts were felled with steel axes (Raymond 1982:28). Based upon the data that have been obtained, it seems conceivable that these corrals were used by local Shoshone peoples for pronghorn drives, as the earliest government land survey maps for the area include both features, referring to each as an "Indian Corral" (Figzhugh 1884). However, the lack of diagnostic faunal materials presents a major problem in determining the exact activity that was carried out there.

There are various sites in the Great Basin where rock wall alignments are found. Invariably, these features are interpreted as having been used for game drives, with pronghorn, deer, and mountain sheep *(Ovis canadensis)* being the principal candidates. The following examples contain no diagnostic faunal materials which have been found in association with or near the rock walls, and therefore must be interpreted with caution.

The Fort Sage Drift Fence is located in northwestern Nevada, and consists of five separate rock alignments spanning nearly 1,800 m. The walls vary from 20 to 80 cm in height, and extend over three low hills (Pendleton and Thomas 1983:7). Radiocarbon dates were not obtained although an occupational range of from 5000 to 1000 years B.P. was proposed, based on projectile points from the Gatecliff, Elko, Rosegate, and Cottonwood Traingular series which were found there (Pendleton and Thomas 1983:34). Identifiable pronghorn bone was not recovered from either the midden deposit or the surface assemblage associated with the rock walls (Pendleton and Thomas 1983:23). However, pronghorn bone was recovered from the backdirt of a vandalized rockshelter located 1,100 m north of the walls, pointing to the possibility that pronghorn may have been utilized there. A great deal more fieldwork must be done here before it can be conclusively demonstrated that pronghorn were communally hunted at the site.

The Bob Scott Summit rock alignment is found in central Nevada, and consists of two rock walls which range from 60 to 90 cm in height. The east wall extends for 100 m and the west wall runs for only 60 m (Thomas and McKee 1974:4). The space between the two walls covers approximately 330 m. A relative age of from 1,500 to 400 years B.P. has been assigned to the walls, based upon locally accepted chronologies for the Humboldt and Rose Spring projectile points that were found in apparent association with the rock features (Thomas and McKee 1974:9-11). Excavations were not carried out, and no pronghorn bone was recovered from the surface. Despite a lack of physical evidence, Thomas and McKee proposed that pronghorn may have been secured through the use of this rock alignment. This appears to be a tenuous conclusion, as the form of the rock feature alone would cause one to question such an assignment. The feature fails to exhibit many of the formal characteristics that are common to pronghorn drive structures as described ethnographically and archaeologically, including,

but not limited to, the following: a relatively long drive wall or wing, the existence of hunting stations or blinds along the drive fence or near the bottleneck, and a narrow space near where the drive wall ends or where the wings meet.

In direct contrast to the Bob Scott Summit site is a series of rock walls in the Matlin Basin of southern Utah. These features consist of several parallel rock alignments, with blinds occurring in a staggered pattern along both sides, with the longest of these walls covering over 1.5 km (Kelly 1943:32). There are also two V-wing configurations incorporated into the parallel alignments, and appear to post-date the original construction of the gauntlet-like rock walls (Kelly 1943:33). Although no faunal remains were found which would suggest that pronghorn were being herded at this site, one could make a convincing case for such an activity based on the form of the rock features alone, as ethnographic data for this area indicates that the Kaibab Paiute used these types of rock alignments for communal pronghorn drives (Kelly 1964:50).

Explorers' accounts of game drive structures.—In addition to the accounts furnished by Egan (1917) and Kern (1876), several other early explorers of the Great Basin observed aboriginal game drive structures which most likely were used for exploiting pronghorn. A drive fence located north of Honey Lake, in northeastern California was noted by Joseph Bruff (1949), on September 25, 1849. Bruff was a draftsman for the Bureau of Topographic Engineers, and a member of the first organized party to explore the Honey Lake area. This structure was seen in the vicinity of the Mud Lake Basin.

> From the elevated and rugged part of the hills to our right ¼ mile, and extending down, in a slight curve—crossing the road, and along the declivity of the hill in front, just beyond the western edge of the deep ravine, and running down, left to the cannon—a distance of 2 miles from the road, was a singular barrier, formed by the Indians, to pen in, probably, large hares when these hunt them (for there is no other game here). This fence was close and regular, except where travel on the road has prostrated and scattered it—was composed of sage and greasewood bushes, torn up by the roots, and placed close together, roots up [Bruff 1949:160].

In all probability, this structure was used for pronghorn drives by the Honey Lake Paiute, and not for rabbit drives, as assumed by Bruff. The extensive length of the drift fence seems to indicate that pronghorn were driven there.

Another historic observance of a drive fence is found in the report and journal of Captain J. H. Simpson (1876). The Simpson expedition traveled across Utah and into Nevada in search of a direct wagon route between Camp Floyd, Utah, and Genoa, Nevada during the spring of 1859.

> Just after crossing Round Valley we passed through a sort of cedar and sage-brush fence, which must have been about .75 of a mile long, and put up by the Indians. Its purpose, doubtless, was to catch rabbits by the suspension upon it of a net, in the mode explained before, and their attempting to run through it [Simpson 1876:60].

This drive trap was seen in the northern portion of Butte Valley, Nevada, and probably was used by local Gosiute Shoshone groups for hunting pronghorn. Steward's map of villages and subsistence areas of the upper Humboldt River region (1938:141) indicates that communal pronghorn drives were held in the area, and that

> A village of perhaps 10 families was located in the northern end of Butte Valley in a canyon called Natsumbagwic (big water coming down), near the Taylor Ranch . . . The chief was Hugamuts, an antelope shaman who directed local hunts. These hunts were participated in by people from a wide area, including Ruby Valley, Cloverdale Valley, the Spruce Mountains, and elsewhere. BM also described an old woman who

conducted antelope hunts in the Butte Valley, the only instance recorded of a woman antelope shaman [Steward 1938:145-146].

DISCUSSION

From the information presently available, it appears that there was a relatively large pronghorn population ranging throughout the Great Basin in prehistoric times. In many areas, this probably allowed for bi-annual, annual, and semi-annual communal hunts (Steward 1938:175; 1941:219; Frison 1971:267). These drives could have occurred in all seasons, but were most likely confined primarily to fall, winter, and spring; as pronghorn tend to disperse in summer, and this time would favor individual efforts.

Various authors have proposed that communal pronghorn drives often resulted in substantial reductions within local populations, and that many years were required before herds regained sufficient numbers, making it feasible to drive them once again (Egan 1917:240; Steward 1938:33; Shimkin 1947:268). It seems that this concept, for the most part, has been derived and generalized from a few post-contact accounts, such as the one which Egan provided, that refer to historic pronghorn populations which were no longer representative of prehistoric herds. Without a doubt, the influx of European explorers, miners, ranchers, and settlers during the mid-19th century exerted additional predatory and competitive pressures upon the pronghorn herds of the Great Basin. This development must have caused radical changes in aboriginal exploitative patterns, as much of the Shoshonean economy was upset during this period. In precontact times, the plains and grasslands of the Great Basin probably supported substantial populations, making annual drives possible for many groups. Much of the ethnographic literature previously mentioned indicates an annual pattern for communal hunts, and an account of the Grouse Creek Shoshone by Steward (1938:175) lends further credence to this theory: "A new antelope corral was built each year. Hunts were held when antelope went south in the fall, and in early spring . . ." Archaeological evidence is also available to support the proposal of an annual exploitative pattern: " . . . the Eden-Farson site . . . represents a Shoshonean group in an area that supported enough antelope to allow an annual procurement pattern without decimation of the herd." (Frison 1971:283).

The potentially adverse effects of pronghorn drives may have been partially mitigated by the likelihood that during any one drive, an entire herd would not be captured (Kelly 1932:84). In addition to this, there is evidence that young animals were set free at the conclusion of the hunt by some groups (Stewart 1942:335; Steward 1943:267).

Another interpretive model of Great Basin pronghorn exploitation which falters in the light of this new archaeological data is that of Pendleton and Thomas (1983), in which they seem to rely heavily upon negative evidence in maintaining that the use of permanent, labor-intensive artiodactyl procurement facilities diminishes through time in the Great Basin (1983:30-31). Based upon recent investigations in the western Great Basin and southern Plains, this concept appears to be questionable, as three sites (CA-Mno-2122, NV-Mi-5; WY-Sw-304) where pronghorn appear to have been exploited via drive traps date from the terminal late prehistoric/proto-historic period, this being based on both artifactual assemblages as well as radiocarbon dates. In fact, the only irrefutable evidence presently available for labor-intensive, communal pronghorn procurement is that it occurred relatively late in time. Frison observed that "Antelope remains have appeared in archaeological sites since Paleo-Indian times but the only evidence we have for communal antelope hunting is quite recent (1978:252).

Pendleton and Thomas also stated that "We know of no accounts of Great Basin peoples constructing permanent rock wall facilities for communal antelope hunting. During the proto-historic period, it seems that corrals and wing walls were made of only

temporary construction materials . . . " (1983:28). Data from western Nevada contradicts this position, as large portions of the pronghorn corral at Little Whisky Flat were built with rock.

Contrary to the views of Pendleton and Thomas (1983), it appears that permanent pronghorn traps were emphasized during the proto-historic period in the Great Basin. However, this does not mean that the tradition does not have a greater time depth. Many drive structures were built with perishable materials such as juniper and sagebrush, and early- to mid-Holocene period traps may not be preserved in the archaeological record. The lack of preservation of wood in open air sites is a major obstacle in tracing the antiquity of game traps in the Great Basin.

Another aspect of communal pronghorn exploitation which should be emphasized is the amount of planning which must have been invested in such endeavors. A wide channel of communication had to be operative in order for people to meet at the proper place and time, and to either strengthen an old trap with new materials, or else to build a new one. All this meant that a great deal of group effort was necessary before a communal drive could occur. This involved carrying and arranging rocks, felling timber with fire, and collecting and stacking sagebrush and other light materials, and probably took some time to accomplish. this view is at odds with that of Thomas, who insists that "The common Great Basin antelope drive employed walls constructed at the last minute . . . " (1983a:50). The point here is that it was impossible to build a trap in a capricious manner; it took time for people to be contacted and to travel to a designated area to accomplish this goal.

A great deal of information stands to be gained from the study of pronghorn drive sites, as our understanding of the antiquity and regularity of this activity is far from complete. There were most likely regional variations in emphasis and technique, due to factors of both human and pronghorn demography, availability of other resources, and extent of contact with other aboriginal and European populations. Pronghorn were most likely a readily available food source throughout much of the Great Basin in precontact times. However, their quickness and keen eyesight probably aided them in maintaining a relatively stable population in the face of human predation. Given the current data base, communal pronghorn exploitation appears to have been important for Great Basin groups in late prehistoric and proto-historic times.

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AN EXPRESSION OF GRATITUDE

It is a pleasure to acknowledge with thanks Mrs. Alwilda Butterfield Sellers of Tucson, Arizona, for her help with mailing Volume 6, Number 1, the Special Issue of the *Journal*. It was she who stamped, taped, labeled, sealed, and otherwise prepared all those envelopes.

Alwilda taught Chemistry and Biology during the 1930s, 40s, and 50s in Belvidere, Illinois. She also developed a course for students with limited learning abilities, and reports that Ethnobiology, along with The Calendar and Astronomy, was a favorite with the students.

Alwilda moved to Arizona for relief from emphysema and had a second career in the now defunct Department of Botany at the University of Arizona. What a pleasure it was to have a secretary (really a professor's aide) who knew how to spell, for example, scalariform tracheids or parthenogenesis, even if the professor could not. So, thanks again, Alwilda for your continuing interest in science, even after retirement from your second career.

Huastec Mayan Ethnobotany. Alcorn, Janis B. Austin: University of Texas Press. 1984. Cloth; 982 pp., $45.00.

This is a potentially rather valuable book, containing an impressive amount of information and presenting interesting new perspectives on several topics, but published in an unfortunately unpolished form. Printed directly from a typewritten manuscript, it gives the appearance of an encyclopedic dissertation rather than a book for wider distribution. For example, the latter half of the book, consisting of a list of several hundred plant species together with their native names, uses, etc., appears to have been printed directly from the author's database program. As such, it is rather extensively codified and hard to follow. There is, of course, a table at the beginning of the section explaining the codes, but 300 pages of such cryptic material is a trifle overwhelming. Even the textual material in the first half of the book is at points somewhat difficult to follow, since the author presents an enormous amount of material without summarizing it adequately. The main points become lost in a sea of seemingly uncoordinated data. The fascinating section on Huastec mythology is vividly written, but the author switches back and forth between etic and emic perspectives so readily that it is a constant struggle to tell the two apart.

However, if you can sift through the mountains of material, there is much of interest from both an empirical and a theoretical perspective. The primary emphasis of the book is on the relationship of the Huastec to their environment, both as seen by the natives themselves and as viewed by the author. She discusses how the native perceptions of their environment affect their techniques of resource management, and how these techniques in turn affect the flora with which the people interact. She discusses native cosmology, first in general and then in relation to the plant world, then gives a detailed description of the people's methods of agriculture and harvest of non-domesticated plants. The author stresses that many of the so-called "wild" plant species (36% of the 965 taxa collected) are in fact "managed" by the human population to some degree, rather than being left totally to natural ecological processes. Finally, she analyzes the effects that the human management processes and the history of contact with outside human populations have had on the plants, both on the genetic makeup of individual plant species and on the overall vegetation of the region.

The introductory chapter on methodology contains a valuable discussion of the problems faced by etnhobotanical researchers, along with the solutions developed by the author for coping with reluctant subjects. Some of these ideas may be of interest even to experienced field researchers as well as to newcomers.

Alcorn in this book has exhibited mastery of both the botanical and the anthropological sides of ethnobotany. I look forward to her future publications, including (I hope) shorter articles summarizing and synthesizing the findings discussed here.

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SEASONAL ASPECTS OF MAN/CATTLE INTERACTION IN BRONZE AGE WESTERN INDIA

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ABSTRACT.—Pastoral mobility has been implicated in the instability of the settlement pattern of Gujarat, Western India in the second millennium BC. The site of Oriyo Timbo, analyzed to further investigate this relationship, displayed indications of temporary pastoral settlement in its lack of solid architecture, thin deposit, location in a non-agricultural zone, and abundance of animal bones, but other evidence complicated this interpretation. A seasonal determination of the deaths of archaeological zebu cattle was attempted via microscopic examination of annual rings in tooth cementum. Annular analysis revealed a clustered pattern of deaths during the dry season months of March to June. These data suggest that Oriyo was a dry season cattle camp associated with more sedentary occupations in nearby areas. Further interpretation concerns the role of herd migration in regional culture change.

INTRODUCTION

The passing of the seasons gives rise to a profound periodicity in the management of domestic herd animals. Seasonality is evident in a wide variety of activities, but certainly one of the most obvious is the movement of animals between different pasture regions in a well-defined yearly cycle. Movement promotes the well-being of the herd, since grasslands used for any length of time will become degraded if not left to regenerate. Herders who exploit neighboring but contrastive environments may also find that different pastures mature and decay in sequence over the year. Under these sorts of circumstances, movement between pastures maintains grazing quality, and prevents the nutritional plane of the herd from plummeting at regular intervals.

Seasonal mobility can benefit the herdsman as well. Migration allows larger herds than can be grazed on the territory of a single settlement, helping to increase pastoral production. Mobility can also have political advantages, exemplified by the Yomut Turkmen of Northeastern Iran, who use migration to avoid taxes, feuds, and government control (Irons 1974, 1975:70-72). Here, migration is a strategy for successful stockraising as much as stockraising is a strategy for successful migration.

While the numbers and influence of seasonally migrating pastoralists are dwindling in the face of agricultural expansion and government settlement schemes, history records periods when these groups made for a potent force for social and cultural change. The Arabian Bedouin, conquering for Islam, and the Mongol warriors each put their permanent stamp upon the lands of the Old World; before them the Scyths and Cimmerians threatened Classical society, while the Biblical Amorites ravaged the landscape of Mesopotamia.

In prehistory, pastoral migration has been associated with the earliest domestication of herd ungulates. Neolithic sheep and goats of the Iranian Zagros are thought to have been led between summer campsites in the mountains and winter villages in

the lowlands (Flannery 1965:1250; Mortensen 1972:296). This cycle may have been simply the mobile component of a sedentary agricultural economy, involving village livestock under the care of village shepherds, but the existence of a more specialized pastoral nomadism as early as 6000 BC has been suggested by the remains at Tepe Tula'i on the Khuzestan Plain (Hole 1974a, 1974b).

The economic and political significance of mobile pastoralism, its impact upon the historical record, and its presumed antiquity suggest that seasonally migrating pastoralists have played major roles in a wide variety of prehistoric contexts. This paper aims to investigate that premise, with reference to Bronze Age South Asia. Specifically, it will examine the issue of seasonality in zebu cattle herding, as it pertains to the archaeological record of Western India in the second millennium BC. Seasons of death of cattle at the pastoral site of Oriyo Timbo will be estimated from patterns of annual rings in dental remains. Season of death will in turn be used to infer the portion of the year in which cattle herds passed through this settlement. This study attempts therefore to reconstruct one segment of the yearly round of a group of cattle pastoralists, in hopes of creating insight into the general herding pattern of the time.

A PROBLEM IN SOUTH ASIAN CULTURE HISTORY

The seasonal analysis of Oriyo Timbo is designed to shed light on the role of pastoral migration in the Bronze Age settlement dynamics of Gujarat State, India. During the late third and early second millennia BC, Gujarat witnessed oscillations over time in site counts, site size, and the character of the settlement grid (Possehl 1980:49-67). At this time Gujarat was an important region of the Harappan Cultural Tradition, comprising the Indus Valley Civilization (ca 2500-2000 BC) and the simpler farming communities which preceded and followed it. The chronology of Harappan Gujarat is defined by ceramic changes, grouped into four phases, at the site of Rangpur in the area of Saurashtra (Fig. 1; Rao 1963). In phase IIA, contemporary with the Indus Civilization,

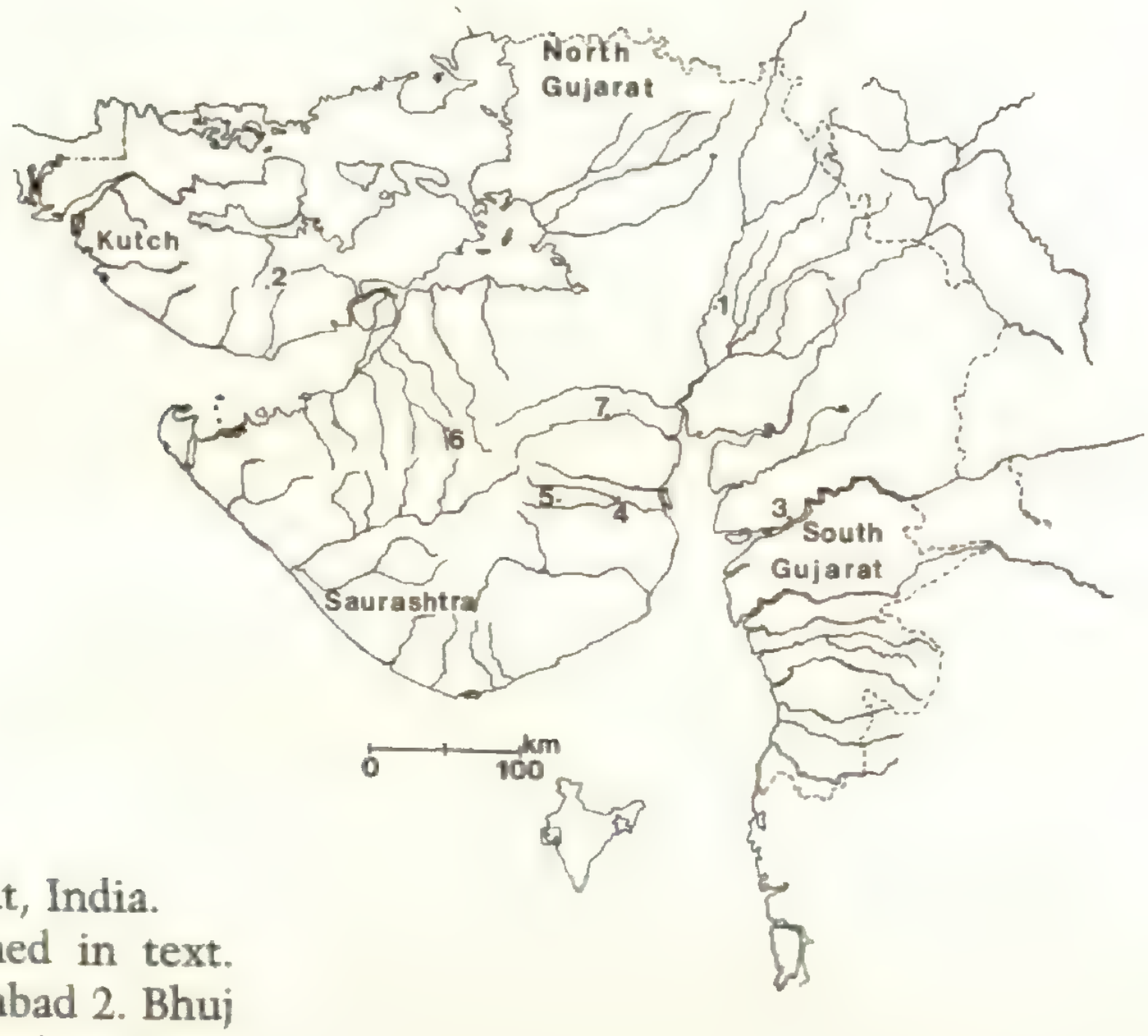

FIG. 1.—Gujarat, India. Places mentioned in text. Key: 1. Ahmedabad 2. Bhuj 3. Broach 4. Nesadi 5. Oriyo 6. Rajkot 7. Rangpur.

a cluster of towns and cities was situated in Kutch, while settlement was sparse throughout the remainder of the state. North and South Gujarat bear no firm trace of permanent occupation, nor does much of Saurashtra. In the phases that follow the collapse of the Indus urban centers (Rangpur IIB and IIC), settlement undergoes a dramatic transformation. The number of known sites increases greatly in all parts of Gujarat but Kutch. Settlement expansion is especially evident in Saurashtra in the form of an integrated system of towns and villages. This trend is short-lived however, because the next period, Rangpur III, is marked by a decline in settlement. Site counts drop, from over 120 to nearly 30, and average site size also decreases where this information is available. (Site counts are current to 1980. Many new sites have been discovered since, but the trends in settlement remain unchanged. See Bhan 1986.)

The decline in settlement in Rangpur III seems to have intensified to the point that any permanent occupation continuing beyond this phase is unknown. Excavated sites with Harappan occupations are generally abandoned at the close of the Rangpur sequence (ca 1400 BC; Possehl and Rissman in press). Those that are not exhibit stratigraphic gaps or other types of material discontinuities between the Harappan and Iron Age periods (Sankalia 1974:383; Mehta *et al.* 1975:4). Firm evidence of village settlement does not re-appear until shortly before the Mauryan period of the late fourth and early third centuries BC, and may be closer to the time of Christ (eg. Mehta 1968). Thus the Rangpur III period not only marks the end of the Harappan Tradition in Gujarat (Harappan material culture continues for varying lengths of time in other regions of South Asia), but also a hiatus in settlement of approximately 1000 years.

These fluctuations may have been influenced by the changing priorities of cultivation versus herding. Possehl (1979, 1986) has suggested that during Rangpur IIA times, much of the open space in Gujarat and elsewhere in the Indus Basin was occupied by migratory pastoralists, whose campsites would not be recognized by archaeologists on survey. Possehl (1986) has also argued that the expansion of settlement in Rangpur IIB/IIC is related to the sedentarization of some of these groups following the introduction of domesticated millets into the subcontinent from East Africa. Finally, a re-emphasis of herding over farming by part of the settled population may have been responsible for the drop in site counts during Rangpur III and after. In this light, further investigation of cultural dynamics in Harappan Gujarat should consider in greater detail the role of migratory pastoralism.

THE SITE OF ORIYO TIMBO

To investigate the significance of pastoral migration in Bronze Age Gujarat, the mound of Oriyo Timbo, near the village of Chiroda in eastern Saurashtra, was chosen for seasonal analysis. Oriyo, excavated in 1981/82 by a combined team from the University Museum, Philadelphia, and the Department of Archaeology of Gujarat State, is dated to the Rangpur III period, the final phase of the Harappan Tradition in Gujarat, by ceramic criteria (Rissman 1985). A number of material characteristics indicate the site may have been a temporary pastoral camp: these are the ephemeral quality of its architecture and habitation deposit, its anomalous location relative to other, presumably agrarian, sites in the region, and faunal evidence of significant quantities of cattle remains.

The temporary character of the occupation at Oriyo is suggested by a lack of solid architecture. Although much of Oriyo has been damaged by the plow, undisturbed Rangpur III habitational material was excavated from one stratum. Preservation of features and artifacts in this stratum was quite satisfactory. Among the finds were a series of hearths, both dug into and sitting upon a fossil ground surface, and a number of bovine hoofprints. The hearths, in the absence of any evidence for industrial activity, were apparently for domestic purposes. Firm evidence of domestic architecture was totally

lacking, however. Walls, floors, post-holes and even chunks of daub were missing from the material inventory. In fact, the only possible architectural feature was an arc of stones from a disturbed context that may mark the remains of a tent or windbreak. The depth of occupation residue is also relevant. Although evidence for settlement is spread across four hectares, the average thickness of deposit may be estimated at no more than 60 centimeters.

The notion of temporary residence is supported by Oriyo's location. The site is situated on a plain between the Ghelo and Kalubhar Rivers; the Ghelo passes 8 km north of the site while the Kalubhar flows 4 km to the south. The placement of the site between the two rivers is unusual for the Harappan settlement pattern of the region. Possehl (1980) surveyed the Ghelo and Kalubhar Valleys and mapped 57 sites of all periods from Harappan to late historic. Of 36 Harappan sites discovered, all but Oriyo were situated within 200 meters of the banks of either river. Twelve other sites were mapped between the rivers, but only Oriyo dates earlier than the second or third century BC. Possehl (1980:52) has argued that the river bank location may have been oriented to land kept free of dense vegetation by periodic flooding, allowing the practice of agriculture without the need to clear away undergrowth. If so, then Oriyo's distance from the rivers may imply that ease of cultivating was not a prime factor in its location.

This suggestion is reinforced by a soil analysis carried out by Dr. K.T.M. Hegde of the Maharaja Sayajirao University of Baroda, Gujarat (Hegde, unpublished communication), which indicates that the ground surface upon which the Harappan occupation began was formed under conditions of poor drainage. This stratum is rich in clay and humus, and contains remains of *Lacrominacroix*, a reed that grows in swampy soil. Hegde speculates that prior to the Harappan occupation the area may have been damp for ten months a year. This seems to be due to local drainage conditions, reflecting impoundment of monsoon run-off, rather than climate change. This layer drained and was exposed to weathering for some time before the Harappan occupation started, but in light of the local quality of the marsh sediment, it is a viable possibility that Oriyo remained in proximity to wetlands throughout the Harappan period. If Oriyo was truly oriented toward marsh rather than arable land, a pastoral function would gain further support. In Gujarat seasonal marshes are used extensively for grazing and constitute some of the region's finest pasture (Bose 1975:6; Patel 1977:20).

Finally, the excavated faunal evidence is consistent with a pastoral interpretation of the site. Cattle, presumably of the zebu variety *(Bos indicus)*, outnumber all other identified species by a wide margin, and comprise 75-78% of all bone fragments in every type of context. Other identified species include sheep *(Ovis aries)*, goat *(Capra hircus)* and domestic (?) pig *(Sus scrofa)*, as well as a few wild representatives of bovid, cervid, and small mammal populations. Cattle remains are not only abundant relative to other species; their density argues for a high level of pastoral activity as well. The densest area of the site produced 896 cattle bone fragments representing a minimum of 25 individuals. This figure reflects an excavated volume of about 80 cubic meters. If this density were extrapolated across the entire site area of four hectares, assuming an average 60 cm deposit, over 8000 head of cattle would be projected, albeit over an unknown time span. From a more disturbed operation, 110 bone fragments yielding three individuals were excavated from 60 cubic meters. If this density were extended over the site, a minimum of 1200 individuals would be represented at Oriyo. These numbers cannot be taken too literally, since a random sampling procedure was not utilized during the fieldwork. Either figure strongly suggests, however, that an impressive number of animals died or were slaughtered at the site, and that Oriyo was the location of a sustained and committed, rather than casual, pastoral effort (the density of cattle remains could suggest that Oriyo was a butchery site for the surrounding region, but the distribution of meat bearing elements to total elements indicates consumption was primarily on-site).

Despite these suggestions of temporary pastoralist occupation, other evidence renders such an interpretation rather tenuous. The presence of a cultivated millet, *Eleusine cf. coracana*, in the botanical material (Wagner 1983), and grindstone fragments in the artifactual assemblage indicate that some form of agricultural activity may have been carried out on site. Heavy storage jar fragments and the possible presence of domestic pig, an animal that is not normally herded with cattle, further argue against the migratory nature of the site's inhabitants. Moreover, the arguments for seasonal pastoralist residence cited above are subject to certain weaknesses of inference. For example, the criteria of architecture and thickness of deposit are plagued by biases associated with negative evidence. The disturbed nature of the habitation may conceal the remains of permanent structures, while occupational residue may once have been more substantial, but has since been diminished by erosion. The criterion of location seems overly deterministic as well. There is no inherent reason why the area could *not* have been used for cultivation; indeed, Oriyo is under cultivation currently. Finally there is no way to confirm that the abundant faunal remains do not simply represent the cattle raising component of a sedentary agricultural village. Thus the evidence is equivocal as to whether Oriyo was actually the home of seasonally migrating pastoralists.

To help resolve this ambiguity a seasonal analysis of herd occupation was attempted. If herds were found to be quartered at Oriyo year-round then the interpretation of a poorly preserved sedentary agricultural village would receive support. If herds were found at the site only at certain seasons however, then Oriyo could be designated a stopping point for migrating animals.

Unfortunately, until recently the tools for measuring seasonal herd, as opposed to human, occupation were woefully inadequate. One of the salient characteristics of pastoral mobility is that it often does not involve the entire human population. When herds migrate, it may be under the care of a few herders, while certain categories of people, especially the elderly, infants, and perhaps adult females, remain within a less temporary settlement. Biological indicators of seasonality, such as the bones of migratory birds, remains of commensal rodents (Hesse 1982; Tchernov 1984), and mollusc shells which preserve seasonal growth increments (Monks 1981) have often been utilized to fix the time of year of site occupation. But while these possibly constitute accurate measures of human seasonality, they do not at all measure herd movement. If one wishes to chart the movement of herds one cannot rely on indicators of human seasonality. One must rather measure seasonality directly from the herds themselves.

One direct measure of herd seasonality has received wide application—this is the examination of herd age structure from tooth eruption and wear patterns in mandibular remains, and the determination of season of death by reference to a restricted birth season (Higgs and White 1963; Bokonyi 1972). For example, if sheep remains show that for a high proportion of individuals the lower first molar was erupting when death occurred, then many deaths at the age of about six months may be assumed. If sheep are known to be born in the spring, then it may be further assumed that sheep occupied a particular settlement in the fall (at the least). This technique suffers from poor reliability however, because eruption times in archaeological fauna must be guessed from modern (or several centuries old) data. Rates of growth and development in domesticated herd animals vary widely according to breed and management conditions, thus ages of tooth eruption have changed significantly, but in unknown increments, over time (Silver 1970:295-299). Aging animal remains to three or six month intervals is highly inaccurate when the ages at which teeth erupt in thousand year old specimens must be guessed from modern representatives.

A MORE APPROPRIATE METHODOLOGY

This research utilizes the analysis of annual rings, or annuli (sing: annulus), in the teeth of archaeological faunal remains, for seasonal determination of herding. Examination of annuli can accurately fix season of death of fauna at pastoral sites, indicating the time of year that herds were quartered in a settlement. Such analysis is a direct measure of herd seasonality, but does not share the disadvantages associated with tooth eruption sequences. Moreover, this technique is relatively new, and has not previously been applied to the seasonal analysis of a domestic herd ungulate faunal assemblage. The technique assesses microscopic annuli in a dental substance known as cementum.

Cementum is a mineralized collagen, structurally analogous to bone, which coats the roots of the teeth in all mammals. It is formed continuously over the life of the tooth. The substance is created when cells called cementoblasts deposit collagen around their membranes (Ramfjord and Ash 1979:49), and then infuse this fibrous matrix with crystals of hydroxyapatite (Selvig 1965:423). Annuli in cementum are observable under magnification as narrow, usually dark (depending on the method of observation), highly calcified bands. While there is no well-accepted explanation for their physiological origin, current opinion suggests they arise from periodic variations in build-up of the collagen matrix. Klevezal' and Kleinenberg (1969:22) suggest that a constant rate of calcification, combined with periodically slowed growth of collagen, will generate the narrow yet calcium rich increment. But whether or not it is controlled by the metabolysis of protein, the fomation of annuli does seem to reflect the seasonally slowed growth rate of the organism.

Since the 1950s a large number of studies have correlated patterns of incremental lines with annual periodicities. It was discovered that among temperate zone terrestrial mammals one narrow, dark line formed in the late fall, winter, or early spring, while during the warmer months a broad, lighter band was deposited (Sergeant and Pimlott 1959; Mitchell 1963; Mundy and Fuller 1964; Novakowski 1965). The narrow band, arising from slowed growth, will be herein termed a rest line; the broad band will be referred to as the growth zone. By counting the number of rest lines, an estimate could be derived of the number of winters an animal had survived, hence it absolute age. In this way wildlife biologists have extensively examined the age structures of temperate zone seal, whale, ungulate, carnivore, insectivore, and rodent populations.

Compared to this abundance of data, research on the dental annuli of tropical and sub-tropical animals has been sparse. Primarily a variety of East and South African bovids (Spinage 1967; Simpson and Elder 1969; Rautenbach 1971; Robinette and Archer 1971; Grimsdell 1973; Simpson 1973; Spinage 1976) have been examined. Tropical animals may form one or two rest lines per year, a pattern that is correlated with the number of dry seasons per annum in their home range. In the tropics it appears that annual rings reflect seasonal variations in moisture rather than winter weather.

Still more obscure are the annular studies of domestic animals and man. Stott, Sis and Levy (1982) met with success in aging three human cadavers from their cemental annuli, claiming a high degree of accuracy. Grue and Jensen (1979:37) also observed incremental lines in human teeth, but it was not possible to isolate primary lines from secondary laminations and consequently no age determination was possible. Similarly, it was concluded that annuli could not be correlated with seasonal cycles in Danish dogs kept as house pets (Grue 1976:4). On the other hand, domestic sheep from New Zealand, and Danish dairy cows and pigs were successfully aged, despite the provison of fodder throughout the year and sheltering during the winter (Saxon and Higham 1968, 1969; Rudge 1976; Grue and Jensen 1979:37).

ARCHAEOLOGICAL APPLICATIONS

Archaeologists developed an interest in annular analysis beginning in the late 1960s (Saxon and Higham 1968, 1969), and broadened the scope of inquiry to include season of death as well. The first seasonal determinations were published by Benn (1974) and Kay (1974), both using white-tailed deer from Woodland sites in the Midwest. In the last ten years additional seasonal and aging studies have been carried out on archaeological remains of several species of seal (Spiess 1976, 1978; Fletemeyer 1977; Morrison 1983), caribou (Spiess 1979:148, 191; Gordon 1982), musk ox (Savelle and Beattie 1982), and cattle (Coy, Jones and Turner 1982). This body of research has shown that annular analysis is a viable technique for both age determination and estimating season of death of archaeological fauna.

Season of death of an individual animal is assessed by examining the outer edge of the cementum deposit, the layer in formation at the time the animal died. If the outer layer was a rest line, death during the winter (or dry season) would be indicated. A growth zone at the outer edge of the tooth would imply that death occurred in late spring, summer, or early fall (or the rainy season). Greater accuracy has been claimed (Spiess 1976; Savelle and Beattie 1982:125; Gordon in press), based on a comparison of the width of the outer layer, either rest line or growth zone, with the widths of the analogous inner layers. For example, an outer growth zone that is half the width of the inner zones might suggest that the individual represented died mid-way through its growth season, or in mid-summer for a temperate zone animal. An outer growth zone as wide as the inner zones would indicate death after a completed growth season, perhaps in the autumn.

When the seasonal data for each tooth are aggregated, the result is a slaughter schedule that indicates the relative proportions of the fauna that died in each part of the year. The schedule is then analyzed for clustering and gaps in season of death. A uniform pattern would indicate slaughter throughout the year. A sharp concentration of slaughter times, in the absence of evidence for death at other times of the year, would instead point to a marked seasonality in killing with further consequences for pastoral organization.

PREPARATION AND RECORDING OF SPECIMENS

Annuli in this study were examined in dental thin sections, observed with polarized light using a petrographic microscope, a standard procedure for archaeological specimens (Morris 1972). Sections were prepared in the following manner: conjoined teeth were sawn apart with a hacksaw, leaving the jawbone intact around the roots. Individual teeth were then impregnated with epoxy. Teeth were sectioned longitudinally along the lingual-buccal plane with a Buehler Isomet slow-speed rotary saw and the sawn face was glued to a glass slide. The tooth was sawn a second time on the Isomet, parallel to the slide and as close to the face of the slide as possible. The specimen was then ground on a wheel and finally by hand on a glass plate until a thickness of 35-40 microns was attained.

Slides were observed using a Vickers petrographic microscope under magnification of 50 to 100x. For each tooth the number of rest lines was recorded as well as the character of the outermost band. The latter variable was recorded in one of four ways. When the outermost layer of cementum was a rest line, a death in the season of slowed growth was noted. Second was the case in which a rest line was outermost but traces of a growth zone could be seen forming on portions of the root. This instance was marked as a death during the seasonal transition from slowed growth to the beginnings of normal

metabolysis. Third and fourth were instances in which the growth zone was the outermost layer of cementum. This zone was distinguished as being in its early or late phase of apposition by comparison of the outer and inner growth zones. If the outer growth zone was one half or less the widths of the inner zones, a death in the early part of the growth season was recorded. If the outer growth zone was greater than half the widths of the inner zones, a death in the latter part of the growth season was noted.

THE CONTROL SAMPLE

The first step in any annular analysis of ancient fauna must be a corresponding analysis of modern animals, of identical species and habitat. This is due to uncertainty about the causes of variable cementum growth rates that give rise to annulus formation. Relationships between climate, diet, hormonal cycles and cementum formation are too obscure to correlate the annular sequence of any animal with a specific environment based simply on theoretical expectations. It is important to recall that annuli will form in humans, housepets, and domestic animals in the absence of environmental variation, even if they do not always assume a periodic pattern. Therefore, before any annular analysis is attempted on an archaeological sample, the exact seasons at which annuli form in the ancient fauna must be previously known through empirical study of a modern population of recorded age and dates of death. Once the seasons of rest line formation have been determined for the modern sample, these data will serve as a seasonal control to which the archaeological material is calibrated, obviating the need to rely on the murky relationships of climate, nutrition and metabolism.

Certain limits to data collection precluded a perfect match of species and habitat between the modern control sample and the Oriyo material. Given Indian sensitivity to the cow it was neither wise nor practical for the control sample to utilize the dental remains of *Bos indicus*. Therefore the mandibles of water buffalo *(Bubalus bubalis)*, a closely related member of the tribe of Bovini, were substituted for those of zebu cattle in the control study. Other herd animals were easily available, so that a sample of sheep and goat teeth was also acquired. A number of pig mandibles were collected to complete the range of domestic ungulates typical of Western India.

Habitat was also imperfectly matched to the ancient sample but in an insignificant fashion. The problem of environmental change through time is not serious, for there is solid evidence that the modern climatic regime of Western India is little changed from that of the second millennium BC (Ghosh and Lal 1963; Singh *et al.* 1974; Seth 1978). But while some of the control specimens were collected in Gujarat, practical constraints necessitated that most of the sample of modern teeth come from the area around Delhi. Environmental differences between Delhi and Gujarat include marginally more precipitation in the capital during the monsoon and in winter as well, somewhat colder winters, and a more variable length of day. The pattern of seasonality however, with monsoonal rains from June to September, a warm dry period until November, cool weather from December to February, and hot dry weather from March to early June, is identical in both places. It was therefore expected that the pattern of annulus formation would not vary significantly between the two regions, and indeed this has been confirmed by analysis.

Along with species collected and locality of collection, the date and age of death were recorded. The teeth of freshly killed animals were gathered during the months of November, February, and July. Age at death was estimated from the tooth eruption tables provided by Silver (1970) for sheep, goat, and pig, and from MacGregor (1939; cited in Grimsdell 1971:37) for water buffalo. One sheep specimen in the advanced adult stage was aged by tooth wear according to the scheme of Payne (1973:293). In all, 15 animals, consisting of six buffalo, three pigs, four goats, and two sheep were examined for incre-

mental lines. If this range of species agreed in the timing of rest line formation, confidence would increase that *Bos indicus* follows a general Western Indian domestic ungulate or bovid pattern.

ANNULAR RESULTS OF THE MODERN SAMPLE

The incremental pattern as recorded from modern animals of known age and season of death clearly suggests that rest lines form under seasonal influence. Of the seven animals killed in November or February, all had a majority of teeth with nearly completed growth zones at the outer edge of the cementum (Fig. 2). Of a total of 37 sections from seven animals, 32 agreed with this pattern. In these Western Indian animals, the months of November through early February fall into the latter portion of the growth season.

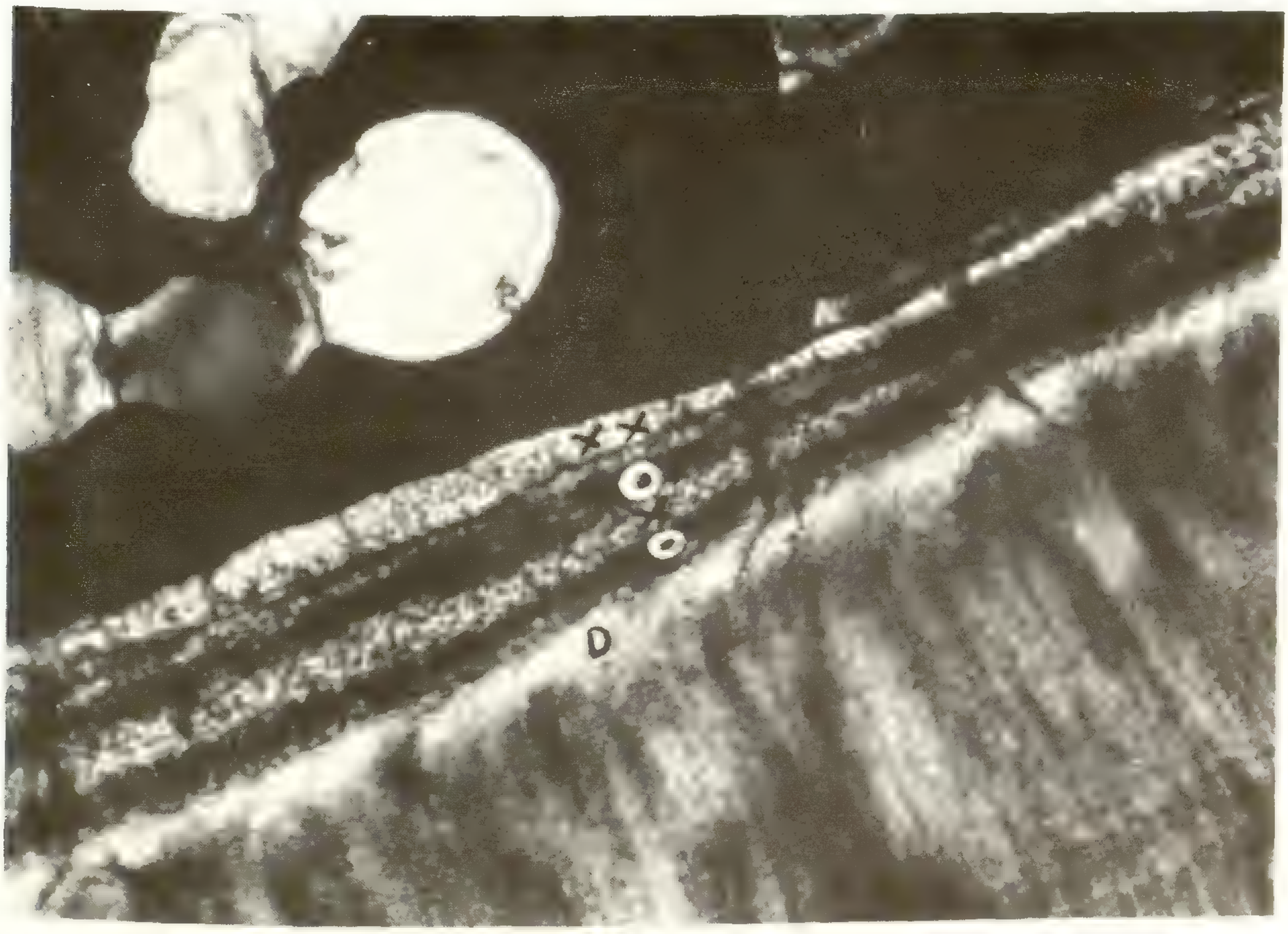

FIG. 2.—Section of deciduous lower third premolar from water buffalo killed in February. O = rest line, X = growth zone, XX = outer growth zone, D = dentin. Magnified 44x.

The animals killed in July are opposite in their indications. Of 24 teeth from eight animals, 19 had the beginning of a growth zone forming at the outer edge of the cementum (Fig. 3). This strongly implies that the month of July falls within the early portion of the Western Indian growth season.

If July marks the beginning of the growth season, and February falls in the late growth season, the time of year during which rest lines are formed can be bracketed sometime within the months of February to July. This result is very satisfactory in light of our knowledge of bovine physiology as it is affected by the Western Indian climate. A definite 'comfort range' exists for tropical cattle that is related to air temperature. As the thermometer climbs about 80°F (27°C), homeostatic mechanisms of cattle are activated,

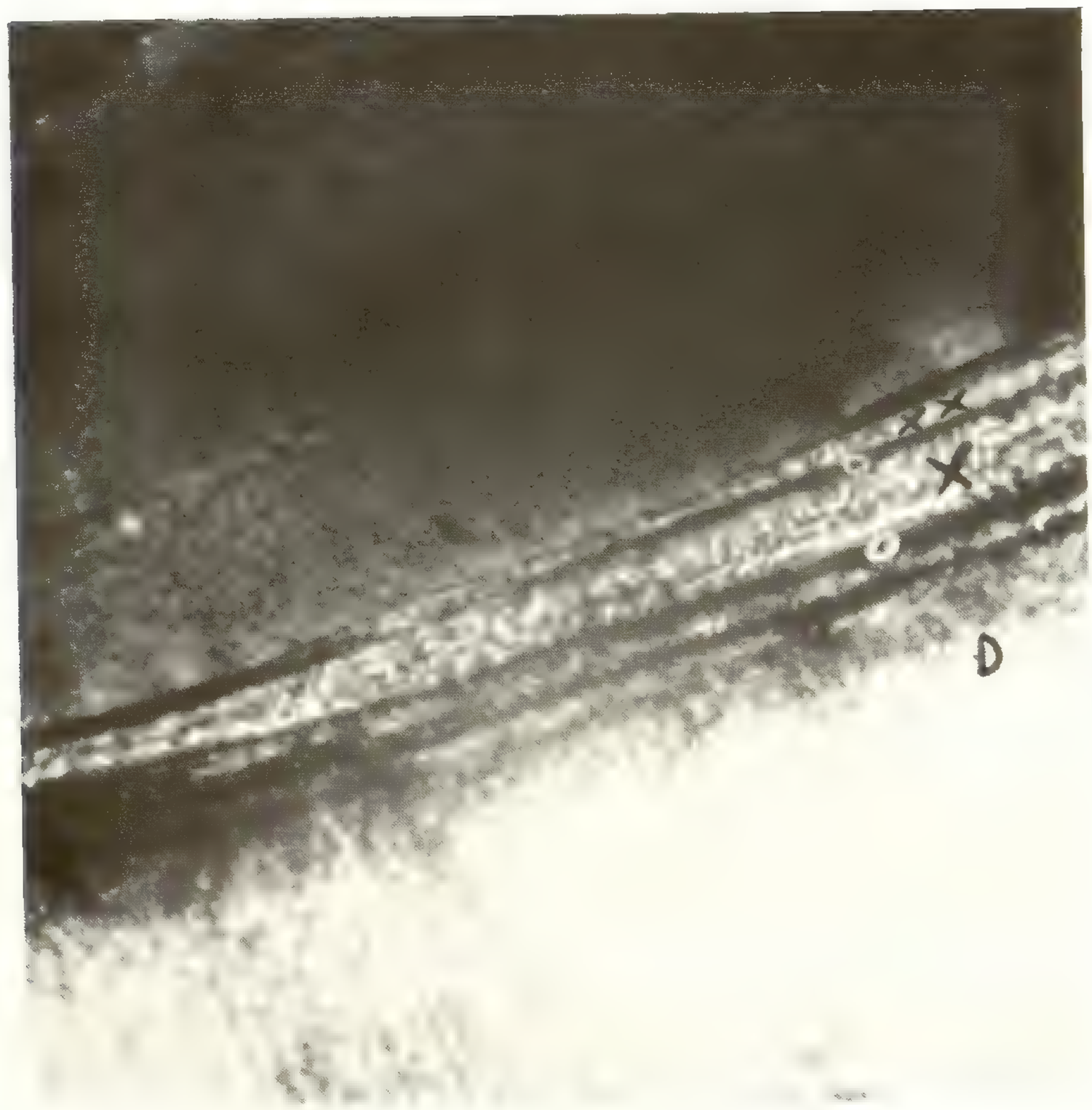

FIG. 3.—Section of lower first molar from pig killed in July. O = rest line, X = growth zone, XX = outer growth zone, D = dentin. Magnified 70x.

causing respiration and vaporization rates to increase (Williamson and Payne 1968:10). When the temperature rises above 95°F (35°C), these mechanisms begin to fail; as a result rectal temperatures rise, food intake declines, *metabolic processes such as milk production and growth slow down*, and a loss in body weight may occur. Studies suggest similar connections might be found for pigs, and possibly for sheep (Williamson and Payne 1968:10-11). In Gujarat and most of Western India mean maximum temperatures rise above 95°F in the months of March or April, and remain above this threshold until mid-June (Government of India 1964). The annular results agree perfectly with this climatic cycle.

Nutritional stresses are also at a maximum at this time. In Western India precipitation is most heavy from late June to mid-September. Between September and June, rainfall is extremely scarce. As the dry season progresses, water sources diminish and vegetation grows brown. The most difficult part of the dry season is just before the rains begin again; at that time, with moisture resources at a yearly minimum, the temperature climbs to its yearly maximum, and through increases in evapotranspiration adds signficantly to the loss of water. During these months, when moisture and pasture quality are both at their lowest point of the year, nutritional stress would be expected at its greatest level.

As animals killed during the months of March, April, May and June were not included in the sample, the precise times between early February and mid-July when the apposition of rest lines starts and stops cannot be further detailed. It is probably safe to assume however, that archaeological animals with outer rest lines were slaughtered in the four month period from March through June.

THE ARCHAEOLOGICAL SAMPLE

Archaeological cattle teeth from Oriyo were mainly recovered in a loose state, detached from mandible and maxilla, though a good number of teeth had fragments of jawbone adhering to their roots. All excavated teeth with roots remaining in place, a total of 62 from 52 sets of individual or paired teeth (some teeth were recovered in connected pairs), were used for the analysis.

Teeth of sheep/goat were mainly recovered intact within mandibles. Mandible halves from the left side of the jaw were used in most cases to ensure that each data point derived from a separate individual. Whenever possible, the first molar was selected for annular analysis. The first molar erupts earlier than the other molars and is coated with a greater amount of cementum, hence it displays a clearer annular pattern. Several fragmentary mandibles required the sectioning of other teeth than the first molar.

SEASONAL DETERMINATION OF ARCHAEOLOGICAL FAUNA

Of 62 cattle teeth from Oriyo, of which 20 were known to be pairs from the same animal, 36 teeth produced clear data on seasonality. Of these 36 teeth, eight were from pairs that had been separated.

The 36 teeth with seasonal data, representing 32 separate data points, show unequivocal clustering of slaughter in the season of rest line formation and shortly after. Of these 36 teeth, only four indicated death in the late growing season, approximately from October to February. When the sample is discussed below in relation to possible bias from differential preservation, it will further be asserted thxt three of these teeth are probably unreliable in any case. Six of thirty-six suggest death in the early part of the growth season, perhaps from the end of June until September/October. A portion of these teeth may also be suspect from the standpoint of preservation. Nine more teeth evidenced death at the end of the rest season and the beginning of the growth season. Death for these animals may have come at the end of June or the beginning of July. Finally, 17 of 36 teeth indicated slaughtering took place in the season of rest line formation from March to mid-June. In all, 26 of 36 teeth clearly show a clustered slaughtering period at Oriyo between March and July.

This cluster becomes even more obvious when the effects of burial and recovery are considered. A possible source of bias in the Oriyo materials is the differential preservation of tooth cementum on the roots of archaeological teeth. The outer layer of the cementum, containing the seasonal data, is the most likely area of the substance to be damaged in a buried context. When this is the case the seasonal indication will be misleading. The best way to guard against this is to segregate teeth into those that have been protected from abrasion to the roots by an adhering fragment of jawbone, and those that have unprotected roots. When compared, any discrepancy between the two groups should be noted. The Oriyo material illustrates this problem well. Below is a comparison of the two groups:

	Season of Death			
	rest	rest/early growth	early growth	late growth
Protected roots	12	4	2	1
Unprotected roots	5	5	4	3

While the protected growth is highly clustered about dry season slaughtering, with 16 of 19 teeth within this cluster, the unprotected group indicates a rather uniform slaughtering pattern throughout the year. Divided in this way, the teeth would appear

to come from two different populations. This discrepancy should be interpreted as indicating the effects of attrition upon unprotected teeth, and should lead to a greater weight placed upon data from teeth with roots enclosed by bone.

Although the sample is much smaller, the seasonal indications of sheep and goat remains parallel to those of cattle, indicating a clustered dry season slaughtering pattern for these animals as well. Of ten firm seasonal data points, five indicate death in the period of rest line formation, two show slaughter at the boundary of the rest and growth periods, two fall into the early growth season and one marks a death in the late growth season. These proportions follow those of cattle closely enough to suggest that the seasonal slaughtering schedules of both large and small stock were identical, clustering strongly within the dry and hot period of March through June, and trailing off into the start of the monsoon in late June and July.

SEASONAL OCCUPATION VERSUS SEASON SLAUGHTER

While is seems clear that cattle and sheep/goats were slaughtered with a pronounced seasonal emphasis, it remains to be argued that such a pattern can indicate the actual season of occupation by domestic herds and herdsmen. The extension of reasoning from seasonal slaughter to seasonal occupation is based on a number of assumptions. One must accept that all dental remains have originated from animals that were slaughtered in the vicinity of the site itself, and that each individual was a member of a herd at the time of its death. These assumptions will place the herd at the site whenever deaths are evidenced. They will be violated if, for example, teeth were used as ornaments or tools and carried from place to place, animal heads had been carried to settlements from remote pastures, or if animals had been singled out of a far-off group, led to the site alone, and then slaughtered. (There is no evidence of the use of teeth for ornaments or tools at Oriyo, while the distribution of post-cranial bones suggests that entire animals are represented in the faunal remains.) If these assumptions are accepted, it follows that annular indications of death in the spring and summer will place the herd inside a settlement during the spring and summer, at the very least. Annular indications of death in every season constitute strong evidence that one is either studying a regime of sedentary pastoralism, or that the site was used seasonally but in random fashion through time.

This relationship is but one side of the coin however, because an absence of annular data for a particular season is no guarantee that animals had been removed to distant pastures; it simply means that they were not being killed. This complexity arises because animals are productive capital that must not be butchered aimlessly. Cattle are far more valuable for their milk, tractive power, and reproductive potential than they are for meat. For this reason slaughtering is generally restricted to non-productive animals, such as males in excess of breeding requirements, barren females, and animals approaching a natural death. Limits to the numbers of such animals force pastoral managers to be extremely conservative and reserve slaughter for special occasions, feasts, rituals, important visitors and the like, or for times of need when other resources are less abundant (e.g. Evans-Pritchard 1940:26; Schneider 1957:289; Spencer 1965:2; Cunnison 1966:30; Dyson-Hudson and Dyson-Hudson 1970:110; Monod 1975:94-98; Smith 1978:81). In areas with a marked ritual season or a pronounced season of scarcity, slaughter itself thus takes on a seasonal cast. Hence, an issue critical to the analysis is how one distinguishes animals that are present at a site, but not slaughtered, from animals that are not there at all.

One approach to this problem (see Rissman 1985 for additional suggestions) compares the slaughter schedules of a series of animal species, each of which might be expected to diverge in seasonal aspects of management or exploitation. For example, the slaughter patterns of domestic herd animals can be counterposed to those of wild species in the

faunal assemblage. The same seasons of death for hunted and herded fauna could support an argument that the site from which they derive was unoccupied during other times of the year. In contrast, indications of year-round hunting would complicate any assertion of seasonal herd migration, although technically it would not refute such an assertion. A similar approach entails the comparison of season of death between domestic herd animals and more desentary domesticates such as dogs, cats, and pigs. Again, agreement in seasonal slaughter would reinforce an assessment of pastoral mobility, and divergence in season of death would serve as a warning signal.

A final comparison, with which the Oriyo data were evaluated, can be made between slaughter patterns of large stock, such as camels or cattle, and small stock like sheep and goats. Herds of small stock exhibit higher growth rates than large stock (Dahl 1979:270), and have less production capacity per animal; therefore the unit value of a sheep or goat is much less than that of cattle. The expenditure of a single sheep or goat is a relatively minor decision compared to the slaughter of a larger animal, and ethnographically small stock are butchered with greater frequency, and more informally, than large stock (Evans-Pritchard 1940:26; Schneider 1957:288; Spencer 1965:2-3; Cunnison 1966:30; Dyson-Hudson 1966:45; Monod 1975:94-95; Smith 1978:81). When a clustered slaughter pattern for large stock occurs alongside a more diffuse pattern for small stock, it may indicate a situation in which sheep and goats were butchered more regularly while cattle were reserved for special occasions. If, however, small stock exhibit a clustered season of death, corresponding to that of large stock, it may imply that both types of herd animals were away from the settlement except during the time of clustered killing. The Oriyo data suggest a slaughtering cluster of similar timing and magnitude for both large and small stock, from March to July in each case, which supports an argument for seasonal occupation of animal herds instead of simple seasonal slaughter.

An additional comment concerns the distribution of occasions at which butchering would be appropriate. While these occasions may be concentrated in particular seasons year after year, certain celebrations and rituals, such as birth ceremonies or funerals, as well as butchering in anticipation of natural death, would tend to have a more random occurrence. There may also be cultural preferences for meat intake that result in a low level of killing throughout the year (Smith 1978:80; Deshler 1965:165). For all these reasons slaughtering patterns can be assumed to cluster in certain parts of the year, but to persist in reduced frequency in other seasons, providing a relatively good measure of migratory herd movement even without the use of corrective tests. The rather obvious clustering of Oriyo herd animal deaths in the final months of the dry season seems quite convincing in this respect.

INTERPRETATION

The results of the annular analysis strongly reinforce the evidence of architecture, depth of deposit, site location and faunal remains in suggesting that Oriyo was a temporary, seasonal campsite of pastoralists. A reasonable reconstruction of lifeways at the settlement centers upon the arrival, sometime in the hottest part of the dry season, of cattle herds, accompanied by smaller herds of sheep and goats, led by a group of herdsmen. The animals may have been brought to Oriyo to take advantage of pasture and water offered by nearby marshland. The human residents used ceramics, manufactured chipped stone tools, and ground cultivated grain at the site. It does not appear that the site was occupied dj ring most of the yearly agricultural cycle (the normal growing season lasts from June to November), nor does it seem that it was situated in an agricultural zone; thus it may be argued that grain was carried to the site from another location in an unprocessed form. It is probable that a number of milch cows provided

dairy products, and perhaps oxen were bled, while meat consumption of surplus or dying cattle was augmented with some hunting. The high density of cattle remains may further indicate that feasting took place as well. The herdsmen and their charges left Oriyo shortly after the first rains had commenced in late June, when pastures in less favored areas had begun to grow verdant. The site was apparently revisited in the dry season for a number of years, allowing the build-up of a thin, diffuse cultural deposit that eventually covered four hectares.

This interpretation is corroborated at the site of Nesadi, located near the point where the Ghelo and Kalubhar Rivers meet in eastern Saurashtra. Excavation revealed the rammed earth floors of several ephemeral structures associated with Harappan ceramics and the bones of cattle and deer [sheep/goat?] (Mehta 1984). Nesadi is located in a marshy area that floods during the monsoon, drying out several months after the rains have ended. These characteristics suggest that the site was not in a dry farming zone, and was seasonally occupied sometime within the dry season. The excavator is of the opinion that Nesadi was a cattle camp. If so, the data from Nesadi and Oriyo imply that in this region, dry season pastoral migration to marsh areas was a general aspect of Harappan herding strategy.

This derivation of regional herding practice, while useful in itself, sheds only limited light on the total pastoral system. A dry season camp represents a small fragment of the annual migratory cycle. In addition, no pastoral settlement, no pastoral economy is self-contained, thus research such as this begs the question of the wider subsistence economy within which pastoralism functions. That the Oriyo inhabitants were in contact with a more settled world is suggested by certain artifactual and biological evidence. Remains of millet do not seem to have been cultivated at the site. They may have found their way to Oriyo either through exchange with more settled communities or through transport to the site by herdsmen who were themselves agriculturalists at other times of the year, in other types of settlements. Remains of pig were judged to be domestic based on a single measurement of a third molar; these animals are unsuited to a life of mobility, and if domestic, probably arrived at Oriyo via the same routes as the grain. There is no evidence at the site for ceramic manufacture, so the pottery, including large storage jars, may also have been carried in from a more permanent residence.

Eight settlements nearby and contemporary to Oriyo are known from Possehl's survey of the Ghelo and Kalubhar Valleys (Possehl 1980:44). The closest to Oriyo is less than 5 km away. As mentioned previously, the sites are located on the banks of either river. Their size distribution is similar, ranging from one to four hectares and lacking in thick cultural deposit. Given their location in a presumed agricultural zone, there is no reason to doubt that these river bank sites were functionally equivalent, small-scale cultivating communities, able to supply the Oriyo inhabitants with agricultural and other products.

These settlements may have removed their cattle, along with some unknown part of their population, to marshlands in the dry season, implying that Oriyo was a satellite camp of the river bank villages. On the other hand, the small size and thin deposit of the river bank sites suggest that a temporary, pastoral character for these too cannot be ruled out. It is also possible that the Oriyo inhabitants came from farther away than the Ghelo-Kalubhar Valley, as part of an extended seasonal round. These issues will not be resolved until one of the river bank sites is excavated, its level of agricultural commitment estimated, and its seasonal character assessed.

FURTHER HYPOTHESES

Despite these unknowns, it is clear that pastoral mobility was one component of the Harappan economy, as well as a determinant of the settlement pattern, most

obviously during the dry season. Its importance may have been considerable in the districts around Oriyo, where agricultural uncertainty is a severe problem. Eastern Saurashtra receives an average of 60 cm of rain per annum. This figure is deceptive because rainfall is very erratic from year to year. The vicinity of Oriyo experiences crop failure due to drought an average of one year in six (Government of Gujarat 1969:212). Another significant feature is the erratic distribution of precipitation over space. When one district is experiencing drought, an adjacent area is likely to be blessed with rain. This is illustrated in Fig. 4, which plots the yearly rainfall of four cities, representing North Gujarat, Kutch, Saurashtra and South Gujarat, from 1901-1950. In 28 years out of 50, some portions of Gujarat received below average rainfall while other areas were above average. In only 13 of 50 years did all four cities receive less than average rainfall simultaneously. Thus, the Harappan farmer of Eastern Saurashtra could gather two things about the monsoon: that with relative frequency, he would lose his crop and his livelihood, and that conditions would probably be better over the horizon.

In light of these environmental characteristics, pastoral migration may have represented a major safety valve for Harappan cultivators. In years of poor rainfall cattle would be utilized as a mobile productive resource, the only one that could accompany

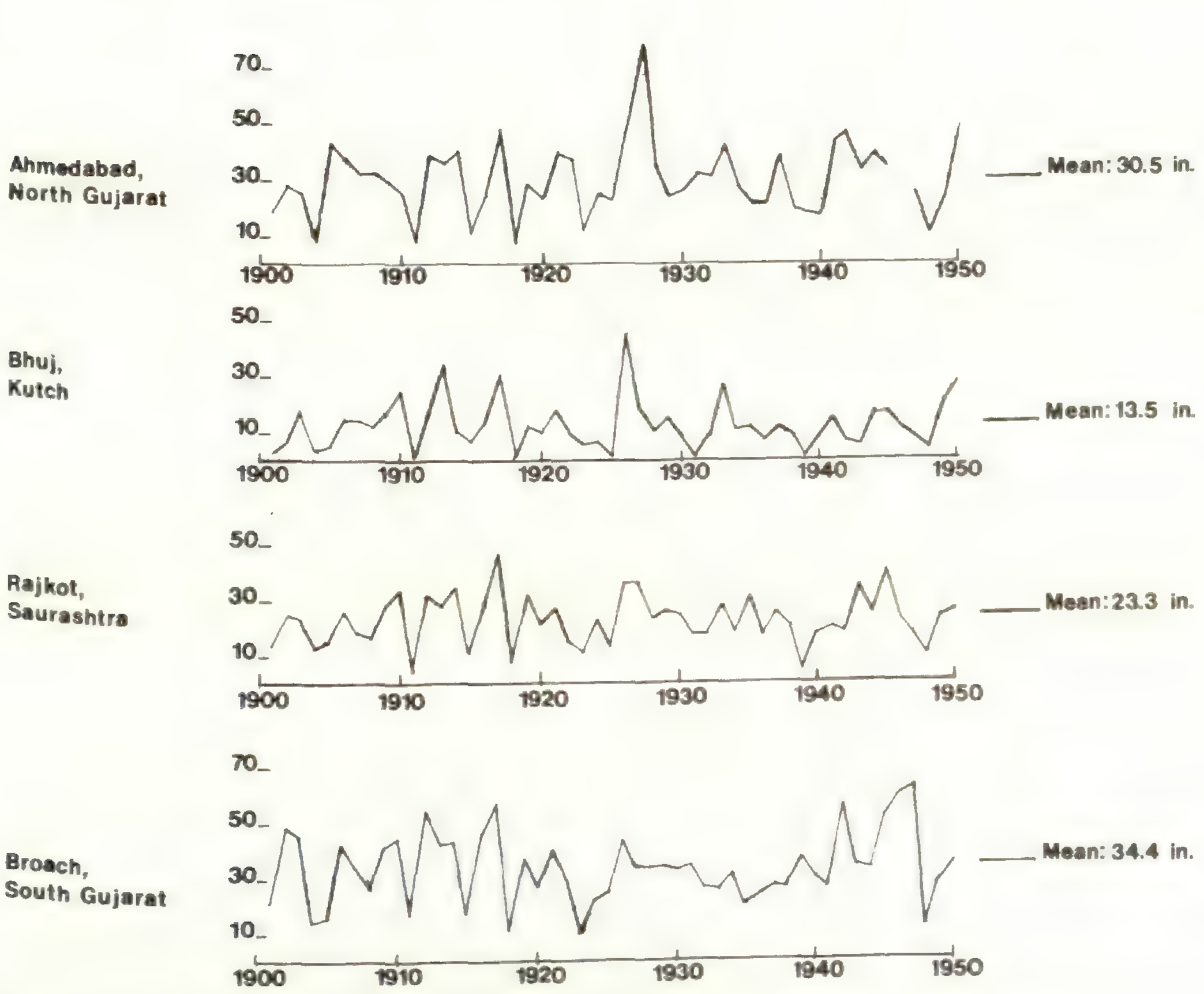

FIG. 4.—Yearly rainfall in inches for four cities in Gujarat, 1901-1950. Source: India Meteorological Department 1970.

a human population to more congenial surroundings until it could return to its land. One might further postulate that the importance of cattle as a mediator of agricultural risk meant that opportunities for nomadization were continually present in Harappan Gujarat. Cattle managers normally try to keep larger herds than is necessary for subsistence, partially to guard against disastrous losses from unforeseen calamities (Monod 1975:117; Dahl 1981:204). Under these conditions herd size is prone to grow past the threshold at which all of the animals can be kept in the vicinity of a single settlement. If this occurs, the herd owner must decide whether he will dispose of some of his animals, entrust them to a herder, or conduct them on a migratory round himself (Horowitz 1975:397-400). The desire of the herd owner to personally manage his stock, combined with the need for large numbers of animals, can thus lead to increases in mobility, or nomadization.

If this hypothesis has any merit, it will allow one to flesh out Possehl's concern with the instability of Harappan settlement, which culminated in the disappearance of Harappan material culture from Gujarat. We can envision a contradiction in settlement: on one hand, millet agriculture provided a plentiful source of calories in good years, encouraging sedentism. On the other, the need to keep large herds as a subsistence hedge must have exerted continual pressure for pastoral migration. One might imagine that during the Rangpur IIB and IIC periods, when settlement expanded rapidly, cultivation was a relatively successful strategy, but by the Rangpur III period and afterwards, migratory herding had gained the upper hand. I suggest that the timing of these shifts may be related to a third factor that mediated the tension between cultivation and herding, this being political administration.

One traditional role of government in South Asia has been the provision of relief during times of agricultural failure. The *Arthashastra*, a manual of political conduct dating back 2000 years, directs the king to set aside half the royal stores as insurance against public calamity (Bose 1961:146), while a third century BC inscription from Bengal records the measures taken by authorities to combat famine by issuing rice from reserves (Wheeler 1968:136). There are no records of this responsibility from earlier times. The Harappan distribution system seems to have been quite sophisticated however, implied by finds of rather standardized artifacts at far-flung outposts, in total covering a larger area than any contemporary polity. It is quite reasonable that subsistence goods as well as more durable items circulated in this system. The erratic spatial pattern of rainfall would favor redistribution, since a drought in one area was often balanced by adequate rain in another, enabling a regional authority to allocate its own resources in a more even fashion.

Harappan administration reached its highest point during the Indus Civilization (Rangpur IIA in Gujarat), but Rangpur IIB/IIC evidences some political complexity in the form of fortified settlements, public architecture, long distance trade (e.g. in gold), and vestiges of writing and measuring systems. Some of these traits persist in the Rangpur III period but the details of the transition are too poorly known to draw any conclusion of administrative trends. The relationship of political organization to subsistence and settlement must therefore remain only a hypothesis until testing is complete. The possibility is fair however, that there was a steady erosion in administrative capacity in concert with the decline of Indus urbanism, and a corresponding downward trend in the strength of the redistributive economy.If one of the functions of redistribution was to even out the erratic pattern of agricultural production, the risks of cultivating would have increased. Increasing agricultural risk would have enhanced the importance of mobility as an adaptive strategy, promoting the value of livestock. This would have encouraged increases in the cattle population, and the reluctance to reduce herd size as it approached local limits could have precipitated the nomadization that is postulated for Rangpur III and after. A telling point in this regard is that permanent

settlement returns to Gujarat only with the re-appearance of centralized political authority in the late first millennium BC.

CONCLUSION

This paper has shown that seasonal aspects of man/cattle interaction can be identified in Bonze Age South Asia, that annular analysis is indispensable in this regard, and that migratory herding can be hypothesized to have great importance in the Harappan Tradition in Gujarat. Annular analysis is not only useful in South Asian archaeology, but is widely applicable to pastoral sites in other regions and other times, as dental remains of herd animals are generally abundant at these types of settlements. The method is relatively new, used for seasonal determination since 1974, and, to my knowledge, never systematically applied to a pastoral economy before the present study. Research into this topic and its capacity to illuminate herding strategies in South Asia and elsewhere deserves to be continued and intensified. Our grasp of seasonality in ancient man/animal relationships has much room for improvement, and a wealth of data, in the form of archaeological dental remains, lies in storerooms and within unexcavated settlements awaiting the analyst.

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Herbal medicine has generated a fair amount of controversy over the years. Its advocates recommend it as a more traditional, natural method of curing, safer and more wholesome than modern Western medicine. Its detractors, on the other hand, denounce herbal medicine and folk healing in general as dangerous, misguided, and unscientific, and maintain that it should not be seriously considered by any educated person. As with many controversies, the truth lies somewhere in between the two extreme viewpoints. Some herbal remedies used by folk healers do indeed perform the tasks attributed to them, in some cases more efficiently and more gently than refined modern drugs; in fact, herbs are the ultimate source of many of the refined drugs preferred by Western practitioners (e.g. *Ephedra* and *Digitalis*). Other herbal medicines yield no positive benefit whatsoever. Some are dangerous poisons, especially in large doses, while others completely lack any discernable physiological effect, though they may possibly have some salutary psychological effects as placebos or as parts of more encompassing healing rituals.

This book represents an invaluable source of information which may help researchers and health practitioners alike to ascertain the true physiological effects of a wide variety of plant species. The book concentrates on plants of dubious value, rather than on those obviously beneficial or dangerous; in fact, the author's original title for the book was "Borderline Herbs". The author predicts in the introduction that he will be criticized for not including plants which are not "borderline", and rightly so. What is perfectly obvious to one person is very often completely unknown to another. Making the book more inclusive would have added to its value as a reference volume.

Some 365 species of vascular plants are discussed, most of them accompanied by line drawings. They are listed alphabetically by scientific binomial, with common names and families listed in the index. For each species, the author discusses the traditional folk uses for the plant, active ingredients which have been identified in the plant, what effects these chemicals have on the body, and what potential problems may arise from excessive doses. There are no written descriptions of the plants, either technical or otherwise, nor any kind of a key to aid identification. This is a decided drawback since it will frequently force users to consult other reference manuals or to trust someone else's identification.

At the end of the book are five tables which supply additional information and make it easier to locate data discussed elsewhere in the book. The first table lists 1983 prices for each herb (in dollars per kilogram, from each of three different North American herbal supply catalogs), and the author's subjective evaluation of the drug's safety. He categorizes each herb on a scale of 0-3, with 0 being very dangerous, 1 more dangerous than coffee, 2 about on a par with coffee in its level of potential danger, and 3 being less dangerous than coffee. To these opinions of his own, he adds similar evaluations of two other writers on the subject, Varro Tyler, whom Duke views as a conservative author, and Jeanne Rose, whom he considers liberal. The very fact that the three writers disagree on their evaluations quite frequently serves to illustrate the difficulty and uncertainty inherent in such crude measurements of safety. All the evaluations are based on ingestion of the same dosage (one cup of tea made from the herb); this may also be somewhat misleading since different drugs yield their best effects at different strengths. Even the most beneficial compounds are dangerous when taken at improper doses. Toxicity is, of course, only half the story; the author does not devote as much space to discussing efficacy of the herbs as he does to their potential ill effects.

The second table contains an extensive list of phytotoxins along with their physiological effects and their distribution in the genera of vascular plants. The third contains much the same information about plants and their toxic components, but alphabetized according to genus instead of by compound. The fourth lists pharmacologically active phytochemicals and their general uses. The fifth and final table is entitled "Proximate analysis of conventional plant foods", but includes some information on vitamin and mineral content as well. This may be valuable in assessing the potential beneficial effects of herbs, since the main value in some treatments may indeed lie in curing nutritional deficiencies rather than in providing some more exotic drug in the conventional sense of that term.

Despite these minor flaws in organization, the book nevertheless represents a significant contribution, one which will settle many questions of the safety and efficacy of herbal medicines. It is much too technical for the average lay person, but it may help health professionals evaluate traditional practices. Students of medicinal ethnobotany may also find the book useful for gauging how much of the benefit from traditional treatment is due to active phytochemicals and how much is due to social and psychological factors.

The book also contains a valuable list of some 353 references which will help anyone wishing to get a start on the enormous amount of literature on the subject.

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AGRICULTURAL SITE SELECTION AMONG PERMANENT FIELD FARMERS: AN EXAMPLE FROM EAST KALIMANTAN, INDONESIA

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ABSTRACT.—Traditional cultivators usually examine a number of environmental variables before choosing a site for agricultural production. In contrast to most studies of site selection, which focus on procedures followed by shifting cultivators, this article discusses the criteria employed by a population of permanent field cultivators of East Kalimantan, Indonesia. The Lun Dayeh, irrigated rice farmers of the Kerayan Subdistrict, differ from shifting cultivators in many of the criteria they employ in selecting a site, in their pattern of long-term observation of selected factors, and in their willingness to substantially alter the site. Important in their choice of a potential agricultural site is an assessment of the amount of labor that is required to render that site acceptable for permanent cultivation.

INTRODUCTION

Many detailed studies of traditional farming systems have noted the great competence of tribal and peasant cultivators in judging and distinguishing the subsistence potential of particular parts of their territories. It has been shown, for instance, that most farmers do not select sites for new agricultural fields randomly, nor for arcane magical reasons. Farmers choosing cultivation sites review both past and present conditions; they recall the cropping history, if any, of an area and carefully examine the standing vegetation—its age and composition—before selecting a location for a new year's field (Conklin 1957, Clarke 1971, Watters 1970). Sound judgement of soils and their capabilities for agricultural production also enters into decisions on siting of farms (Conklin 1957, Richards 1939, Allan 1965), as do several other concerns, among them relative distance to farms of relatives and co-workers (Freeman 1970, Deegan 1973), and proximity to other fields worked by the same household (Padoch 1982).

Geographers and anthropologists are not alone in noting the knowledge that many pre-industrial populations have of the physical characteristics of their environments. Development planners, among them agronomists, agricultural economists, and others have concluded that especially in the agriculturally marginal tropical environments that are often largely unknown to university-trained scientists, indigenous tribal peoples can be extremely competent consultants on the use and development of their own territories (Brokensha *et al.* 1980).

Most studies of traditional soil classification and agricultural site selection have focused on the procedures by which shifting cultivators choose an area for their annual farms. With few exceptions (e.g. Conklin 1980), little has been written on the knowledge of soils and other environmental variables that preindustrial or pre-literate permanent field cultivators possess. This is at least partly because many swiddeners annually choose where to farm, while most permanent field farmers, as the designation implies, cultivate the same fields each year. The latter tend to live at far higher population densities and farm what land they already own. Patterns of land tenure therefore tend to be by far the most important determinants of where a farm is situated. Most permanent field

farmers choose a new site to put into cultivation very infrequently and thus researchers rarely have the opportunity to observe the procedures involved in selecting new areas for permanent fields.

"Permanent field farming" encompasses many different types of resource use and therefore the environmental variables that farmers consider vary greatly from one agricultural system to another. In all instances, however, decisions of where to site a permanent field take into account long-term agricultural suitability; swiddeners commonly need judge only whether an area will support cultivation for a year or two. Thus choices made by permanent field farmers will differ from those of swiddeners in two general ways: first, a great deal of long-term observation is usually involved in making selections of permanent sites. And second, since they regard the new field as a long-term investment and frequently are willing to change several environmental factors to make the site more acceptable, the choices made by permanent field cultivators typically include an estimate of the amount and type of labor needed to put a particular area into a useable state. Shifting cultivators engage in far less environmental manipulation other than the clearing of sites.

THE LUN DAYEH OF NORTHEASTERN BORNEO

The Lun Dayeh of East Kalimantan in Indonesia practice both shifting and permanent field irrigated cultivation of rice and a number of subsidiary crops[1], and are noted throughout much of Borneo for their abundant annual rice harvests (Padoch 1985). The success of Lun Dayeh farming has been attributed by many not familiar with the group's homelands to extremely favorable natural conditions for agriculture, particularly to fertile soils. A closer examination, however, shows that the Kerayan Subdistrict, where most of East Kalimantan's Lun Dayeh live and farm, is a highly varied region, blessed only in very limited areas with exceptionally good conditions for rice production. The success of Lun Dayeh agriculturists is more reasonably attributed to their detailed knowledge of the environments they exploit, to their sound judgments of where to site their fields, and to their good management practices.

The site selection criteria employed by one Lun Dayeh population have been described (Deegan 1973) and are, according to the report of the researcher, largely similar to those used by swiddeners throughout the tropics; the height or age of vegetation covering a plot is apparently the prime factor considered. These farmers, recent immigrants to the Lawas Damit Basin of Sarawak, are, however, quite atypical among Lun Dayeh agriculturists. They are mainly shifting cultivators and have few irrigated rice fields. The wet rice cultivators living in the Kerayan uplands of East Kalimantan, Indonesia, the traditional Lun Dayeh homeland, observe other environmental factors and emphasize different criteria in choosing sites for their irrigated permanent fields.

The Kerayan Subdistrict.—The Kerayan Subdistrict, located in the northeastern corner of the District of Bulungan, along the border with Malaysia, comprises an area of 3,930 km^2 (Fig. 1). Topographically, it is a region of hills, often steep, divided by several broad, flat valleys. The area was described in general terms by Schneeberger in 1945. Since then, no specific information on the topography or geology of this region has been published. The geology and soils of the neighboring parts of the Malaysian state of Sarawak were recently surveyed, mapped, and described (Eilers and Loi 1982); the maps, unfortunately, stopped at the international frontier. Although a government-sponsored preliminary geologic survey of the Kerayan Subdistrict was carried out in 1980, the information gathered has not been made available to the public.

The volume on northern interior Sarawak and information given by the geologic surveyers in the Kerayan (J-L. Nagel, pers. comm.) both suggest that the area is underlain

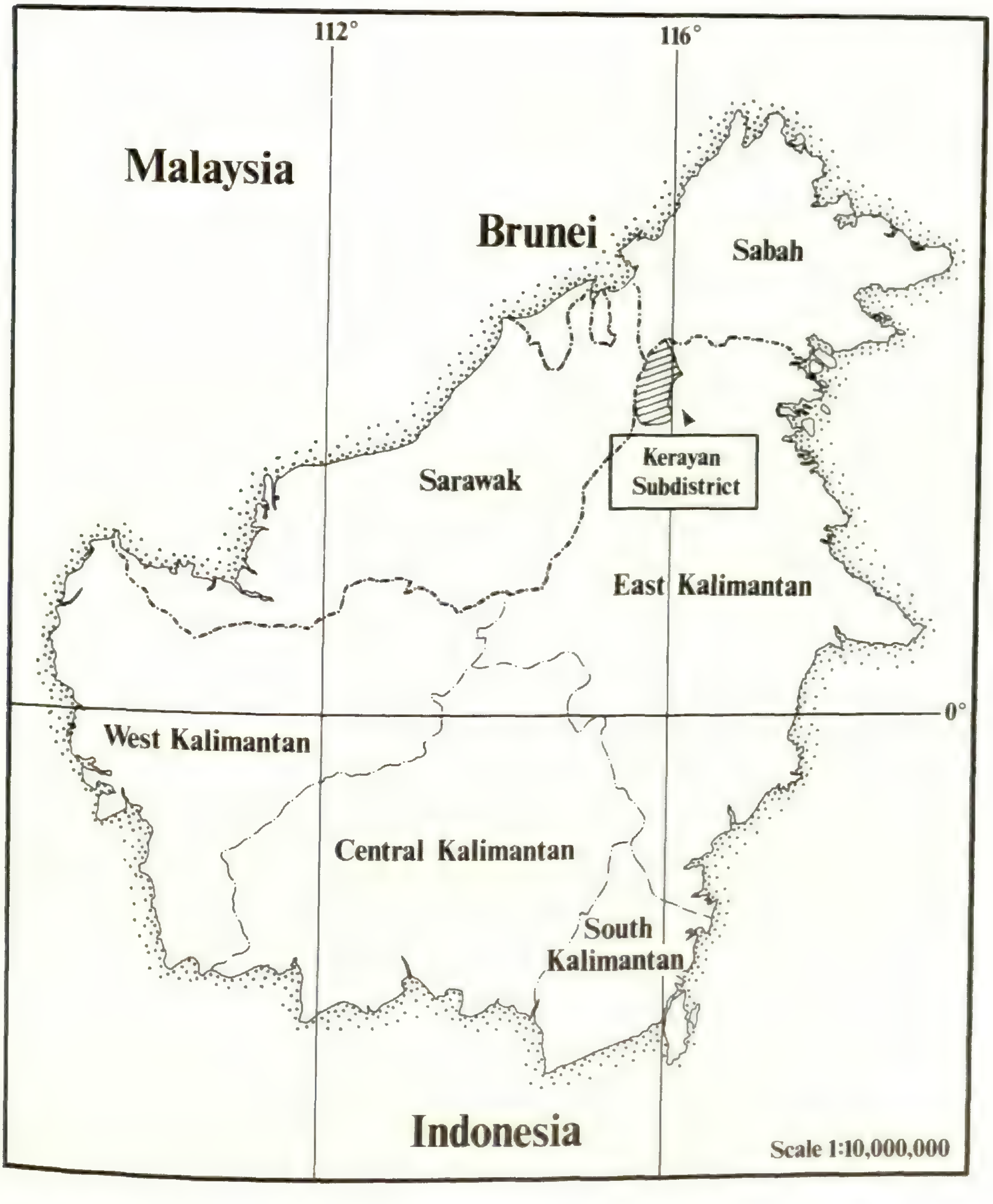

FIG. 1—Map of Borneo (Kalimantan) indicating the location of the research area.

largely by alternating bands of sandstones and shales, which give rise both to the broken topography and the variable soils of the region. The principal river draining the area, the Kerayan, and its tributaries, the Lutut and the Bawan, have their headwaters in the Subdistrict and are surprisingly slow flowing, silt-laden streams. Only farther downriver does the Kerayan become a rushing rapid-filled river more characteristic of upland Borneo waters (see Schneeberger 1945, for details on the area's topography).

The underlying parent materials—sandstone and shale—have contributed to the formation of sandy and clayey soils characteristic of the Kerayan area. A chemical analysis of representative soils of the region (Padoch 1981: Appendix 1) showed that the soils

of the Kerayan are typically very poorly suited for agriculture. They are very low in pH (averaging about 4.0 in the uppermost layers, with a range between 4.8 and 3.3) and have extremely high levels of aluminum saturation. Suggestions that the productivity of Lun Dayeh agriculture stems largely from the exceptional fertility of the soils are not supported by these analyses.

The settlements and the permanent rice fields of Lun Dayeh farmers in the Kerayan are located in valleys lying approximately 900 m above sea level. Some of the major hills rise perhaps 1000 m above the inhabited valleys and are typically cloaked with mature hill dipterocarp forests. Several areas of lower heath vegetation, known as ***kerangas***, dominated by species of rhododendron, are also encountered in the region. All these areas are situated on very acid white sand soils and many are poorly drained. Tea-colored "black-waters" typically flow off these areas. The local inhabitants deny that they ever cleared or otherwise disturbed these patches of low vegetation, and indeed they always avoid such sites in choosing areas for agricultural production.

Considerable stands of secondary vegetation in various stages of regrowth can be seen, particularly around a number of the newer villages. These patches are evidence of recent clearing for shifting cultivation. No extensive areas of grass, apart from the "village greens" around which most settlements are grouped, are found in the Subdistrict.

The population of about 10,000 persons is composed almost entirely of farming households. Larger villages comprising several hundred inhabitants are located in the few particularly broad and flat valleys of the region where extensive areas of irrigated rice land are also found. Narrower valleys tend to be farmed by members of smaller settlements. The human population density for the entire Subdistrict is only about 2.2 persons per km^2, and considerable areas for future expansion of both wet and dry rice agriculture continue to be available.

Most Kerayan farmers who mainly cultivate permanent wet rice fields use the same fields each year, and thus exercise little choice in locating their annual cropping areas. Because of population growth, however, some Lun Dayeh households are expanding their fields or making new ones and therefore are choosing new areas for agriculture. Moreover, in response to a government sponsored resettlement program, as well as to a desire on the part of many Lun Dayeh to live nearer to sources of market items, medical care, and schools, several villages have moved to new locations in the last fifteen years. The inhabitants of these recently resettled villages, among them the community of Long Umung along the Lutut River, are now selecting areas suitable for permanent field cultivation and are constructing new irrigated and bunded pond-fields. Thus, an unusual opportunity to observe how Lun Dayeh farmers choose sites for new agricultural undertakings, including new inundated rice fields, now exists.

AGRICULTURAL SITE SELECTION

Data concerning the criteria and procedures used for choosing new farming sites were collected in 1980 over a period of nine months of observation and participation in agricultural activities. Informal questioning of a large number of Long Umung's farmers, both men and women, provided the major part of the data presented here. Much information was also gathered not by direct questioning but rather by recording comments made by Lun Dayeh farmers as they went to and from their fields, observing the agricultural potential of areas passed, and as they worked at agricultural tasks. Somewhat more formal interviews were conducted with three persons—Binyamin Barok Siran, the headman of the village of Long Umung, his wife Ripka rigo, and Barok Tai, an older farmer; these three informants contributed a large part of the information and confirmed or corrected much of the data that had been collected by observation. Information on soil types was gathered primarily from these three persons, although many more of Long

Umung's inhabitants participated in soil sample collection, color and texture evaluation, and lengthy discussions of agricultural worth. All interviews and discussions were conducted in the Lun Dayeh language.

Long Umung is a relatively new village having been settled approximately thirteen years ago by Lun Dayeh migrants. When these settlers first arrived at their new site along the upper Lutut, they produced the rice they needed by cultivating impermanent swiddens. Virtually every year since their move to the Long Umung site, however, these farmers have been opening up more and more land for permanent field agriculture and reducing the size of their swiddens accordingly. The area around Long Umung offers considerable opportunities for irrigated field cultivation although abundant room for continued shifting cultivation can also be found.

When determining the use to which a plot of land in the Kerayan should be put, farmers consider many variables. As in other wet rice farming areas (Conklin 1980), the topography, particularly the slope, of the area is an extremely important criterion. The farmers of Long Umung also assess the previous agricultural history of the site, and its vegetative, pedological, and hydrological characteristics before deciding on its use as a permanent, dyked and inundated field, a temporary swidden, a house site, or an area to be left in natural forest vegetation.

To be a prime site for inundated permanent field farming, a plot of land should be located not far from the farmer's home. It should also be flat and extensive, close to a clear stream which flows in all seasons but is not subject to excessive natural flooding, with heavy, fertile soils (as indicated by the soil color and texture and/or by the size and type of vegetation covering the plot), and, if it has previously been cultivated, it should have a history of abundant rice production. Although most plots available for development now do not possess all of these desirable characteristics, a number of environmental factors can and usually are altered to some degree to approach more closely the ideal, even though such alterations are usually costly in time and labor.

Location of fields.—General considerations such as distance from the site of residence, as well as distance from other areas farmed by the same household, are rarely mentioned in published discussions of agricultural site selection. However, since location often influences the time and effort a farmer must put into activities such as travel, crop protection, and the building of irrigation facilities, distance is often important in the decision of where to locate a field whether it is to be a swidden or an irrigated farm. Farmers especially wish to locate permanent fields close to permanent homes. Swiddens can often be made far from the village; a small shelter can serve as a temporary residence for a year or so until that field is no longer intensively cultivated (Freeman 1970; Padoch 1982, on *dampa*, temporary Iban residences).

The recent directive from the Indonesian government to group small villages in interior Kalimantan into larger population concentrations, has met with some resistence in the Kerayan, since for many farmers such a move involves spending additional time in walking each day to and from fields. Lun Dayeh, like many other cultivators also attempt to locate new farms close to other farms being worked in the same season; a wide scattering of fields increases the labor required in protecting crops from the depredations of sparrows, monkeys, and other animals. (The Lun Dayeh construct ingenious systems of alarm devices over large fields which can be operated by just one person sitting in a central location.) If fields are far apart, more labor, for many households a scarce resource, is required for proper protection of fields[2].

Topography, Water, and Drainage.—Swiddeners consider a broad range of topographic conditions acceptable for swidden sites; slopes of up to 40° are often farmed in Borneo. However, in selecting a plot for an irrigated pond-field, minor differences in topography

become important. Flat valleys are ideal and slopes are avoided, if possible, because although topography can be and usually is altered somewhat, a great deal of labor is consumed in this task. Ideally, valleys should also be wide and long so that large areas can be enclosed and served by a single set of engineering works for irrigation and drainage. As the Kerayan Subdistrict is rather sparsely populated, terracing of sloping lands is still minimal. Most farmers continue to be successful in locating plots where farms of significant size can be made with minimal earth moving. Such opportunities are clearly decreasing.

At present Lun Dayeh consider almost level terrain to be a *sine qua non* for establishing a new agricultural plot, another requirement is a nearby source of water of suitable quality and availability. Two kinds of water are distinguished by Long Umung farmers: black (tea-colored) waters and clear waters. It has been pointed out by researchers, particularly those working in the Amazon Basin (Sioli 1975; Klinge 1967), that black waters generally flow off white sand soils and are poor in nutrients and low in pH. Although no analyses were done of Kerayan black waters, their chemical characteristics were assumed to be similar and indeed, the Lun Dayeh, also aware of the nutrient poverty of such black waters, avoid them in selecting areas for the practice of wet rice cultivation. Waters used for irrigating rice fields are always clear.

Availability of water is as important as quality. Farmers casually observe potential areas for wet rice cultivation throughout a year or more to determine whether sufficient irrigation water is apt to be available in all seasons of the agricultural year. Again, as with topography, water availability is a condition which can be altered and generally is changed somewhat by the construction of irrigation canals, bamboo aqueducts, and water gates. All these alterations, of course, require considerable labor and farmers attempt to minimize the changes that must be made by carefully selecting a proper site.

Areas that are naturally inundated can serve as favorable sites for wet rice farming only if the water is not too deep and the area does not flood excessively at any time during the year. Farmers often make note of the depth of standing water on such a site each time he or she passes through the area over a period of several years. Occasionally very deep areas are made shallower by dumping earth into the site, but because of the large investment of labor required for such improvements, deeply inundated areas are generally avoided.

Soils and Vegetation.—While permanent field wet rice cultivators differ from swiddeners in considering factors such as topography and water quality and availability in making site selections, they also take into account soils and vegetation, the two prime criteria that concern shifting cultivators. Their judgements here differ as well: permanent field farmers can and do choose at times to considerably modify the soils of their sites; swiddeners, as a rule, do not. As pointed out in several other studies of indigenous soil classification systems, color and texture of soils tend to be considered most prominently when sites for agricultural plots are chosen (Conklin 1957, Reina 1967, Carter 1967). The Lun Dayeh also focus largely on these two qualities.

Soils are classified by color into "white" ***(mebuda')***, "black" ***(mitem)***, "red" ***(masia')***, and "yellow" ***(mebirar)***. ("Red" and "yellow" are rather poor translations of the last two; the distinction between "red" and "yellow" and ***masia'*** and ***mebirar*** is made very differently by an English-speaking investigator and a Lun Dayeh farmer.) Cutting across each of these color categories are classifications relating largely to soil texture, that is "sandy" ***(mebada')***, "clayey" ***(mesalit)***, "very hard or heavy clay" ***(metueh)***, and "stony" ***(grifere)***. There is also a term, ***meroko***, for a layer or mat of roots that overlies some soils, a condition found in several sites around Long Umung. And there are two other general terms or categories applied to soils: one, ***mebaleng***, denoting a generally fertile and easily worked soil, which is often an alluvial soil and ***metugeh***, a generally infertile,

poor soil, which is often also ***metueh*** or a very heavy clay. A few other soil type names are used to refer to very specific soil types such as ***tana' luyan***, a bright red heavy clay and ***tana' mefayeh*** or ***bada' buda'***, an almost pure white sand, a soil to be avoided in almost any agricultural undertaking.

This classification of soils is similar to the systems employed by swiddeners to find an appropriate site for a temporary field. What does distinguish Lun Dayeh observation and classification of soils from those employed by shifting cultivators is the care these permanent field cultivators take in examining soils in depth. Lun Dayeh farmers are well aware of the fact that soils vary with depth and take into account the possiblity that their efforts at leveling a site may result in their either removing or uncovering a soil layer that is either more or less desirable for rice production. Thus some farmers will dig to see what kinds of soils they are apt to uncover if they remove the top layer of soil in constructing a new field. Commonly occurring soil profiles, however, are rather well known and farmers know what sorts of "B" horizons are apt to be found under which "A" horizons. Thus, an informant stated that although a layer of ***tana' meroko*** or an almost purely organic root mat can be removed when a field is being made, this is rarely worth the effort and thus is seldom done because such root mats commonly overlie very poor ***mefayeh*** soils. It is recognized that changing the soil surface can result in the uncovering of both better or worse surfaces for rice production.

The possibilities for modifying a site's soils do not end with changes that result from leveling the area; particular sites may also be given different treatments depending on their shortcomings. Although Lun Dayeh pond field making involves far less soil preparation than is the norm in irrigated rice systems[3], plots with soils judged to be ***metugeh*** or hard and infertile are sometimes altered by tethering a water buffalo in the spot over a period of a week or so. The buffalo will both puddle the soil and fertilize it by defecating in the field. Thus a particular treatment can make a site more appropriate for cultivation. The shifting cultivator, employing a particular site for only a very limited period generally will not spend the time or resources on fertility enhancement procedures other than the burning of slashed vegetation and perhaps the spreading of the resulting ash.

Invariably the prime cirteria used by a shifting cultivator in determining whether a spot is suitable for a swidden are the height and general composition of the vegetation on it, often indications of the age of the stand. For the Lun Dayeh permanent field farmer height, or age of vegetation is only important if it serves to indicate whether the soils are so poor that nothing other than scrub growth will appear or whether the plot is regularly inundated. The swiddener's preoccupation with the age of the stand which determines the ease of forest clearance, the quality of the burn, and the quantity of the resultant ash, as well as the presence or absence of grasses and sedges that may become important pests do not concern the permanent irrigated field farmer.

Lun Dayeh cultivators survey vegetation to get some indication of the soils as well as to judge the flooding hazard at particular sites. Vegetation is important, as the presence or absence of particular plants indicates a number of conditions, a "gestalt" important for the rice farmer. The particular species that the informants at Long Umung suggested as especially useful for determining whether sites were suitable for the creation of permanent pond fields are listed in Table 1. It is interesting to note that judgements of Lun Dayeh farmers as to the suitability of a site, as shown by the presence or absence of these species of plants, were duplicated by a university trained ecologist well familiar with Borneo. Dr. Paul Chai, Forest Botanist of Sarawak, identified the plant specimens and, relying on his extensive general knowledge of local ecology, independently evaluated each plant as an indicator of agricultural potential. His ratings were exactly the same ratings as those of the Lun Dayeh informants. The nine species listed are those most commonly known and identified. Many Lun Dayeh informants could also name dozens

Table 1.—*Common plant species indicating agricultural potential.*

Species	Family	Lun Dayeh name	Condi. indicat.
Rubus moluccensis L.	Rosaceae	serinit	good
Selaginella aff. *brevipes* Fee	Selaginaceae	gugor	good
Piper aff. *caninum* Blume	Piperaceae	buyu' berek	good
Curculigo borneensis Merr.	Hypoxidaceae	lapa'	good
Cyrtandra trisepala C.B.L.	Gesneriaceae	taneb luba'	average
Lycopodium cernuum L.	Lycopodiaceae	lio fade	poor
Curculigo villosa Wall.	Hypoxidaceae	tamar	poor
Timonius finlaysonianus Hook f.	Rubiaceae	anur sia'	poor
Clethra lonspicata J.J.Sm.	Clethraceae	anur ferian	poor

of other plants, particularly tree species, which are characteristically found in association with known soil types and with known agricultural potential.

Areas that are deemed unsuitable for either permanent or swidden cultivation may still be used as house or village sites or may be left in natural forest. Such areas often still serve as sources of important subsistence products, among them spontaneously occurring fruits, leaves, fern shoots, and fungi. Animals are hunted in these forests, and materials for building, fuel, and handicrafts are collected.

SUMMARY AND CONCLUSIONS

Like shifting cultivators and other traditional agriculturists throughout the world, the Lun Dayeh of East Kalimantan's Kerayan Subdistrict weigh a number of environmental variables before selecting a site for a new agricultural field. While the environmental variables most important to swiddeners, including the composition and height of the vegetation covering the area are taken into account, Lun Dayeh permanent field cultivators tend to be more concerned with other environmental characteristics, including slope, water quality and availability, drainage, and soil quality both on and below the surface. Moreover, they attempt to judge the long-term acceptability of the site. Therefore, continuing observation of an area, often for years before it is first used, is not unusual. While the criteria for an acceptable site for an irrigated farm field are many, Lun Dayeh cultivators assume that some changes will be necessary to bring a field to the condition required. Therefore, a most important consideration is the amount of labor that will be needed to render a site acceptable for pond-field farming. Since population density in the Kerayan is still low and almost ideal areas are available at present, Kerayan Lun Dayeh farmers continue to reject any site that requires major improvements such as significant terracing work, the filling in of sites that tend to flood too deeply, or the digging of long irrigation canals. As most easily worked areas are claimed in the future, what is judged an acceptable site will certainly change, and the construction of ever more labor demanding engineering works will be considered necessary and reasonable.

A rather extreme example of another phase of this process is found among the Ifugao of Luzon. According to the reports of Conklin (1980), the Ifugao, in choosing a site, consider the incline of an area the most important criterion. Since flat or gently sloping lands are no longer available in Ifugao, an acceptable slope is far more steep than any site that the Lun Dayeh would attempt to exploit. Also important is availability of a

water supply upslope from the chosen site and location of materials necessary for the building of terraces in the vicinity of the selected area; these include walling rock, rough fill, and topsoil. In the Ifugao case, cultivators do not expect to find, but rather to create most of the conditions necessary for pond-field farming and their fields, in contrast to those found in the Kerayan, can be considered largely artifacts, since the soil, including subsoil and the entire surface of each terrace, have been created with human labor.

The willingness and ability to alter environmental conditions, which in large measure distinguishes Lun Dayeh pond-field farmers from their swiddening counterparts, is considerably more developed among wet rice farmers such as the Ifugao whose homelands offer fewer unexploited ideal sites for irrigated cultivation. A comparison of labor needed to create pond fields in different areas would be interesting but is difficult because of the great variability of sites available in each location (Conklin 1980; Padoch 1981). However, initial pond-field creation costs as well as subsequent labor for maintenance in regions like Ifugao surely far surpass those found in the Kerayan. With ever increasing alteration of the original environment, the need for human labor in maintenance constantly increases as well. The Lun Dayeh, therefore, in seeking out areas that naturally most closely approach the conditions desirable in an irrigated pond-field are minimizing not only initial construction costs, but also subsequent maintenance.

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NOTES

[1]General descriptions of swidden or shifting cultivation as well as of irrigated, inundated, wet, or sawah systems of rice production can be found in many publications, among them Hanks' *Rice and Man* (1972) and the introductory chapter to Geertz' *Agricultural Involution* (1963). Although the irrigated rice cultivation patterns employed by the Kerayan Lun Dayeh depart somewhat from the classic sawah techniques described in the above works (Padoch 1985), the general characteristics of Lun Dayeh farming, including their temporal aspects, are similar to the well-known models.

[2]For a general discussion on labor requirements in Kerayan Lun Dayeh agriculture see the article by Padoch (1985). Freeman's (1970) and Padoch's (1978) monographs on Iban swiddeners discuss the need for guarding crops from animal depredations in one area of Borneo.

[3]Lyn Dayeh irrigated rice farmers do not plow or puddle their plots, as do most wet rice farmers. For further discussion of agricultural techniques and labor expenditure in the Kerayan, see Padoch (1985).

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ETHNOBOTANICAL NOTES FROM THE VALLEY OF SAN LUIS, COLORADO

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ABSTRACT.—Knowledge and use of plants by the Hispano Americans in the San Luis Valley of south-central Colorado, USA, are the result of selective diffusion of Mexican Spanish traditions and of the adoption of local Native American customs. Of the 66 taxa recorded in the study, the native ***oshá*** *(Liqusticum porteri)* is one of the most important herbal remedies of this once culturally and geographically isolated region. The Pueblo Indian term ***oshá*** is applied also to the European lovage *(Levisticum officinale)* which is cultivated as a substitute for the wild plant. Certain native plants such as *Ribes leptanthum* and *Mentha arvensis* carry introduced Mexican Spanish names and uses. In addition to gathering the plants locally, 22 species are available from commercial suppliers through local pharmacies.

INTRODUCTION

The Valley of San Luis ("El Valle") is located in the Rio Grande Basin near the head waters of the Rio Grande River (Rio Bravo) in south-central Colorado and adjacent New Mexico. The basin floor with an elevation of about 2290 m is covered with dry grass and shrubland interspersed with fields in which vegetables, potatoes, alfalfa, wheat and barley are cultivated. The surrounding higher terrain, the San Juan Mountains on the west and the Sangre de Cristo Mountains on the east, are forested by various communities of pine, oak, aspen, spruce and fir. Both ranges have peaks over 4270 m. The weather in the valley is characterized by short, cool summers and long, cold winters. About 870 flowering plant species are reported in the Coloradoan San Luis Basin (Dixon 1986).

Despite various Spanish explorations of "El Valle" since the late 1500s and colonization attempts since the late 1700s, successful Hispanic settlements were not established until the early to middle 1800s (López T. 1975). The early Spanish settlers were associated with land grant or "mercedes" and attracted Mexican mestizos as workers. The early residents were separated from Mexico except for infrequent commercial or personal visits to Santa Fe (founded in 1598) or to Chihuahua (founded in 1709). Consequently, they were isolated and depended upon the few Spanish and Mexican items they brought with them and upon the local resources. There was little intermingling with Native American groups, such as the Comanches, Navahos and Utes, that hunted in the area. The Spanish Americans remained isolated after the region was incorporated into the United States. Only recently and gradually have they (especially the younger generation) become acculturated into the Anglo-American lifestyle.

The effect of blending the Spanish Colonial heritage and the native customs and the isolation of the early settlers is evident today in northern New Mexico and adjacent Colorado. Natives of "El Valle" speak a distinctive archaic form of Spanish (Cobos 1983). Social activities, holiday celebrations, songs and folktales in the Valley of San Luis reflect local development that stemmed from the original influence of Santa Fe and Chihuahua (Lopez T. 1975). The Hispanic health tradition is characterized by the complex of ***yerba buena, oshá,*** and ***inmortal*** (Schulman and Smith 1962) which is composed of a northern hemisphere herb, *Mentha arvensis* L., and two native plants, *Liqusticum porteri* C. & R., and *Asclepias* sp., respectively. In "El Valle" it is believed that medicinal plants do not have energy until the "bendición" (blessing) in mid July (Darlene Norton, pers. comm.) which coincides with two Catholic saints' feast days: "el día de Santiago" and "el día de Santa Ana" (López T. 1975). Traditionally, the summer and early fall were the important seasons to collect the herbs for storage and consumption during the winter when most the illnesses occurred.

"Remedios" (medicines) consist of plants (which may be employed separately or in herbal mixtures) as well as other items. For example the ***siete flores de casa*** is a variable mixture of flowers from around the house which are crushed and placed in the ear to alleviate ear aches. Other materials may be mixed with the plants or used alone. Animal products include deer blood, baby urine and virgin honey. Home-made vinegar, whiskey and sodium bicarbonate are also employed.

Certain "remedios" such as ***plumajillo*** and ***oshá*** are said to be eaten by deer and goats but not by cattle. Consequently the meat of these animals as well as the goat milk are considered to be healthier.

According to residents of "El Valle," the early settlers attempted to introduce plants from Mexico that were important "remedios." For example ***romero*** *(Rosmarinus officinalis* L.) is mixed in alcohol with ***oshá*** *(Liqusticum porteri)* and is applied topically to treat arthritis and rheumatism. In the past ***romero*** shrubs from Mexico had been planted by the ancestors but never became established in the Valley. Today, the ***romero*** is purchased in the pharmacies which obtain it through commercial suppliers.

"Curanderos" and "curanderas" (also called "médicas" according López T. (1975)) are native healers in "El Valle." Today, they are regarded highly by those who still value cultural medical customs. However, they are the older people and no young traditional medical practitioners are known.

The introduction, adoptation, and rejection of native and foreign plants have influenced the retention of knowledge and use of vegetable resources in "El Valle" by Hispano Americans. Information about these herbs has been passed down over the generations and has been subject to few outside influences until recently. The geographic and cultural isolation offers an unusual opportunity to study the ethnobotanical knowledge that has persisted in the region that was once the northern frontier of Mexico. Lack of cultural interchange between the Hispanic and Anglo peoples reinforced the traditional uses of plants. Today, the rapid acculturation of the Hispano Americans has resulted in decline in the knowledge and employment of herbs.

The few botanical studies (Dixon 1971, 1986; Ramaley 1929, 1942) mainly describe plant communities or are species lists of vascular plants. Although no general ethnobotanical studies have been published for this region of Colorado, the writings of Curtin (1965) and the compilation by Ford (1975) serve as classic references based upon voucher specimens for traditional medicinal plants in adjacent northern New Mexico.

"LA BOTICA"

"La Botica" (the pharmacy) is a special habitat located on a shelf (Fig. 1) in the canyon of La Jara Creek (R7E T34N, section 28; Vicente, Colorado, quadrangle) on the

FIG. 1.—"La Botica" located on the shelf between the top of volcanic rock ridge and La Jara Creek, looking to the west (July 1984).

western side of the valley at an elevation of 2640 m. It has been a local source of medicinal plants for many generations "El Valle" residents. The plants were thought to have been protected (or even planted) by the Indians "por que habia muchos remedios juntos" (because of the local concentration of many medicinal plants) and later encouraged by the early Hispanic settlers. About 15 years ago, the area was overgrazed by sheep resulting in the spread of sagebrush and the disappearance of many medicinal plants. Today the area is dominated by sagebrush *(Seriphidium tridentatum)* and surrounded by scattered clumps of *Abies concolor, Pseudotsuqa menziesii* (Mirb.) Franco, *Populus tremuloides* Michx., *Pinus edulis,* and *Juniperus* spp.

METHODS AND MATERIALS

This study was initiated with three goals to: (1) document the Mexican medicinal plants at the northern limit of Spanish Colonial and Mexican National settlements (Linares and Bye in press); (2) identify the local useful plants of "El Valle" as an assistance to the residents interested in preserving their customs; and (3) obtain voucher specimens of plants used as a "remedios" or folk remedies in a collaborative project with G. Appelt, School of Pharmacy, University of Colorado, Boulder (Appelt 1985).

Collections and interviews during 1983 and 1984 concentrated in the Coloradoan towns of Alamosa (Alamosa County), Capulin, and La Jara (both in Conejos County). Lists of plant names, plant uses and administration, and specimens were obtained from residents, pharmacists and customers purchasing herbs. Plant samples were collected from pharmacies, gardens, fields and forests. Knowledgable local people verified specimens and accompanied us into the field.

Two forms of voucher specimens were made: (1) pressed, dried herbarium specimens in the case of fresh plants and (2) packaged dried plants in the case of material obtained from homes or pharmacies. The herbarium specimens are deposited at the University of Colorado Herbarium (COLO), Adams State College Herbarium (ALAM), and Herbario

Nacional de la Universidad Nacional Autónoma de México (MEXU). The packaged material is deposited in the collections of the Laboratory of Ethnobotany, University of Colorado, and of the Laboratorio de Ethnobotânica, Jardîn Botânico, UNAM. Color slides as well as black and white photographs were made of the plants when possible.

RESULTS

The local plants of importance to the Hispanic residents of the Valley of San Luis are discussed below in alphabetical order by family, genus and species. Because of the limited nature of the study, they do not represent all of the plants recognized or used by the Hispano Americans in "El Valle." Some plants are collected by the users, exchanged within the family and among friends, or purchased in pharmacies. The herbs sold in stores may be collected locally or obtained from commercial suppliers such as Green Mountain Herbs (Boulder, CO), A. López (Belen, NM), Los Remedios de la Gente (Santa Fe, NM), Flores Nacional (Tucson, AZ), Sunrise Teas & Spices, Inc. (Reno, NV), and Peru's (Los Angeles, CA) (see Table 1).

ANACARDIACEAE
Rhus aromatica Ait. subsp. *trilobata* (Nutt.) Weber ***lemita***

Deer are said to be fond of browsing this low shrub found in open sites on the lower slopes and with pinon-juniper. Its red, acid fruits are eaten raw with beans or preserved with sugar for later consumption. *Bye & Linares 12,916.*

APIACEAE
Levisticum officinale Koch ***oshá; oshá del jardin***

At least three generations are thought to have cultivated this perennial herb (Fig. 2a). The leaves are dried, powdered and used as a condiment, often kept in a shaker on the table, and maybe are added to cooked beans. It is considered a substitute for ***oshá de la sierra*** *(Liqusticum porteri).* When plants are hilled in the spring, they produce elongated petioles which can be eaten like celery but have the flavor of ***oshá.*** *Bye & Linares 12,247; 12,946.*

Liqusticum porteri C. & R. ***oshá; oshá de la sierra***

The characteristically odoriferous rootstock of the perennial *Liqusticum porteri* is collected in the spruce forest during late summer and early fall after the leaves have turned yellow. It is the most popular "remedio" in San Luis Valley for treating colds, sore throats and stomachaches. The shavings of the root (Fig. 2b) may be chewed or are infused and the tea drunk. To aid the healing of wounds, the powdered root is applied in a gauze to prevent infection. For fevers, one is bathed in an infusion of the roots. The leaves are dried in the shade little by little so as to not lose their strength. They are used as a condiment alone or with ***orégano*** in bean dishes. As a food additive or as a tea, ***oshá*** is said to aid the digestion. Carrying a piece of it is a practice to prevent snakebites. It is thought that in the past the older people were not sick because they always carried a piece of ***oshá*** in a small bag or pouch. It is believed that one will stay young by taking this root daily. Locally collected roots as well as those from out of state suppliers (who use the name ***chuchupate***) are sold in the pharmacies. *Bye & Linares 12,649; 12,659; 12,262; 12,949.*

TABLE 1.—*Plants from commercial suppliers sold in pharmacies of San Luis Valley. The collection numbers refer to specimens of Bye and Linares.*

AMARANTHACEAE	
Amaranthus cruentus L.; ***alegria***	12,265
APIACEAE	
Ligusticum porteri C. & R.; ***chuchupate raiz***	12,268
ASTERACEAE	
Achillea lanulosa Nutt.; ***plumajillo***; yarrow	12,276
Artemisia mexicana Willd.; ***estafiate***	12,270
Chamomilla recuitita (L.) Rauschert; ***manzanilla***	12,660
Liatris punctata Hook.; ***cachana***	12,661
Pericalia sessilifolia (Hook. & Arn.) Rydb.; ***cachana***	12,668
Thelesperma sp.; ***cota***; Navajo tea	12,646
Ximenesia enceliodes Cav.; ***añil del muerto***	12,647
CHENOPODIACEAE	
Teloxys ambrosioides (L.) Weber; ***epazote***	12,271
Teloxys graveolens (Willd.) Weber; ***epazote de zorrillo***	12,272
FAGACEAE	
cf. *Quercus* sp.; ***encino***	12,648
LAMIACEAE	
Dracocephalum sp.; ***toronjil***; lemon balm	12,227
Lavandula officinalis L.; ***alhcena***	12,266
MALVACEAE	
Sphaeralcea coccinea (Pursh) Rydb.; ***yerba de la negrita***	12,281
RUBIACEAE	
Hintonia sp.; ***copalquin***	12,267
RUTACEAE	
Casimiroa edulis Ll. & Lex.; ***hojas de zapote blanco***	12,282
SAURURACEAE	
Anemopsis californica (Nutt.) Hook. & Arn.; ***yerba mansa***	12,645
SCROPHULARIACEAE	
Verbascum thapsus L.; ***punchón***; mullein	12,654
TILIACEAE	
Tilia sp.; ***flor de tila***	12,278
TURNERACEAE	
Turnera sp.; ***hojas de damiana***	12,269
ZYGOPHYLLACEAE	
Larrea tridentata (Moc. & Sessé) Cov.; ***chaparral; gobernadora***	12,273; 12,651

FIG. 2.—Some useful plants of San Luis Valley, Colorado. Fig. 2a.—***oshâ del jardín*** *(Levisticum officinale)* cultivated in Capulin *(Bye & Linares 12,247)*. Fig. 2b.—shaved rootstock of ***oshâ de la sierra*** *(Ligusticum porteri)*. Fig. 2c.—young leaves of ***dormilones*** *(Rudbeckia ampla) (Bye & Linares 12,944)*. Fig. 2d.—***champes*** *(Rosa woodsii) (Bye & Linares 12,259)*.

ASCLEPIADACEAE

Asclepias sp. ***inmortal***

The thick, vertical root of the perennial ***inmortal*** is said to be collected locally in such areas as Antonito and, taken as tea, is a popular "remedio" for colds and is taken as a tea. It is said to be similar to ***contrayerba*** and pleurisy root. Locally collected roots are sold in the pharmacies. Three species are reported from the Valley of San Luis: *A.*

asperula (Dcne.) Woodson, *A. hallii* Gray, and *A. speciosa* Torr. (Dixon 1986). *Bye & Linares 12,652.*

ASTERACEAE

Achillea lanulosa Nutt. ***plumajillo; plumajillo de la sierra;*** yarrow

The characteristically divided leaves, white flowers and bitter taste signal the presence of the perennial yarrow of open areas on the basin floor as well as of the forested mountains. The aromatic vapor of the infusion of this herb, poleo *(Mentha arvensis)* and ***altamisa de la sierra*** *(Artemisia friqida)* is inhaled to treat "gripa" (colds) and is used in "jumasos" (hot bath with vapors). For fast relief of stomachaches, the leaves are chewed and swallowed or an infusion is drunk. It is also used to treat "el frio en el estomago" (gastrointestinal pain caused by cold air entering the body). The herb tea is consumed to relieve painful and bloody urination. To enrich the blood of those with anemia, the infusion is consumed. This popular "remedio" is available dried in the pharmacies and is derived from local and New Mexican sources. For home storage and consumption, the leaves are collected in August and dried. The plants in the valley floor are called ***plumajillo de la casa*** and are not considered to be as effective. *Bye & Linares 12,655; 12,260; 12,276.*

Artemisia frigida Willd. ***altamisa de al sierra; estafiate; plumajillo de la sierra***

The low, perennial herb *Artemisia frigida* is found on the valley floor and in the forested lower mountains. The bitter, divided leaves are chewed and swallowed to alleviate stomachaches rapidly and to treat "el frio en el estomago." Dried plants from local sources are sold in the pharmacies. For home storage and consumption, the leaves are collected in August and dried. The ***estafiate de la sierra*** is said to be more effective than ***estafiate del campo*** and is most common near dying or dead *Abies. Bye & Linares 12,656; 12,908.*

Chamomilla recutita (L.) Rauschert ***manzanilla***

Home gardens in "El Valle" and northern New Mexico contain plants of this European herb. A tea made from it is said to be effective in relieving "jiel" (bile colic) and gastrointestinal pain. Babies with "chincual" (baby diarrhea) are given an infusion by a dropper. This popular herb can also be obtained at the pharmacies which purchase it in bulk from commercial suppliers. *Bye & Linares 12,660.*

Grindelia aphanactis Rydberg ***yerba de buey***

Disturbed areas in the valley harbor this annual herb. Local, dried plants are sold in the pharmacies. *Bye & Linares 12,663.*

Grindelia squarrosa (Pursh) Dunal ***pega***

An annual herb with yellow flowers *Grindelia squarrosa* is common in recently disturbed sites in the valley though it is thought to be more common in New Mexico. Regular drinking of a tea from the plant is considered to be a good treatment for cancer, especially when taken as soon as the cancer begins. It cleans the person from the inside. It is also helpful in treating stomach ulcers as a tea and sores as a topical wash. *Bye & Linares 12,253.*

cf. *Helianthus ciliaris* DC. ***yerba de buey***

This annual species of sunflower grows on the valley floor and extends into New Mexico from where fresh plants are brought and sold in the pharmacies. *Bye & Linares 12,280.*

Liatris punctata Hook. ***cachana***

Liatris punctata, a perennial herb with purple flowers, is limited to the eastern part of the valley although in the past it was collected in "La Botica" on the western side. To alleviate stomachaches and colic, one bathes in an infusion of the swollen rootstocks as well as drinks the tea. In the past, the rootstocks were used to wash clothes. Dried rootstocks originating in northern New Mexico are popular in the pharmacies and are a traditional component of sterility "remedios" (Linares and Bye in press). *Bye & Linares 12,661.*

Rudbeckia ampla A. Nelson ***dormilones***

The immature leaves (Fig. 2c) of this perennial herb are collected fresh from moist areas along streams in June and are cooked and eaten as a ***quelite.*** The young, tender leaves are preferred because the older, large ones are bitter. Like other ***quelites,*** they can also be dried and stored to be used later by soaking them. They are considered a delicacy although they can be confused with the poisonous ***yerba de peco*** (*Actaea rubra* (Ait.) Willd.; *Cicuta occidentalis* Greene) (Curtin 1965; López T. 1975) and toxic ***yerba de perro*** (unidentified). *Bye & Linares 12,944.*

Seriphidium tridentatum (Nutt.) Weber ***chamiso hediondo;***
(syn., *Artemisia tridentata* Nutt.) ***chamisona; chamiso hediondilla***

The perennial sagebrush *Seriphidium tridentatum* grows in isolated parts of the valley, such as "La Botica," and in denser stands in the southern part of the valley and adjacent New Mexico. It is said to be more common in New Mexico where the foliage is darker in color. The tea and hot vapor bath using the foliage are employed in the treatment of "resfrios" (colds). It is one of the bitter herbs used in a mixture for colds; sugar is added to improve the flavor. Also a tea is drunk to lower blood sugar. *Bye & Linares 12,932.*

Ximenesia encelioides Cav. ***añil del muerto***
(syn., *Verbesina encelioides* (Cav.) Benth.)

Ximenesia encelioides is a common annual herb in disturbed areas near houses and roads on the valley floor and lower mountain slopes. A bath prepared from the whole plant is used to calm the pain of arthritis, rheumatism and "resfriados" (colds). The herb is first boiled in a pot and then the infusion poured into the bathtub. Dried plants from New Mexico are sold in the pharmacies. *Bye & Linares 12,247; 12,648; 12,940.*

BRASSICACEAE

Capsella bursa-pastoris (L.) Medic. ***bolsa de pastor;*** shepherd's purse

Shepherd's purse is a weedy European annual of disturbed areas on the valley floor. Dried plants from local sources are available in the pharmacies. *Bye & Linares 12,657; 12,662.*

CACTACEAE

Coryphantha vivipara (Nutt.) B. & R. ***huevos del Indio***

This low, ball cactus is found on open slopes in the pinyon-juniper woodland. *Bye & Linares 12,936.*

Opuntia polyacantha Haw. ***nopal redondo***

The pads of this cactus of dry, disturbed sites on the valley floor and in the pine forests are consumed to lower blood sugar. *Bye & Linares s.n. (photo).*

CUPRESSACEAE

Juniperus communis L. subsp. *alpina* Celakovsky — ***sabina macha; sabina de la sierra***

The low, prostrate shrub *Juniperus communis* is scattered in the understory of the pine forests. A tea of the foliage is drunk to treat the kidneys when one has painful urination and "flujo de sangre" (excessive menstruation). It is also administered in "jumasos" where by one sits over a basin with the branches in hot water and is covered with a blanket. This ***sabina*** is said to be more effective than the ***sabino*** *(Juniperus monosperma). Bye & Linares 12,261; 12,937.*

Juniperus monosperma (Engelm.) Sargent — ***sabino***

A tea from the foliage of sabino is drunk when it is painful to urinate. This small tree can be found on the lower mountain slopes. *Bye & Linares 12,939.*

EQUISETACEAE

Hippochaete laevigata (A. Br.) Farwell (syn., *Equisetum laevigata* A. Br.) — ***canutillo de llano***

This perennial horsetail thrives along irrigation ditches and other moist areas on the valley floor. The locally collected dried stems are sold in the pharmacies. *Bye & Linares 12,666.*

FABACEAE

Medicago sativa L. — **alfalfa**

Alfalfa, growing both as a cultivated and escaped exotic perennial, is common on the valley floor. The blended fresh leaves are drunk by those with fragile bones. The infusion is used in making a bath for treating arthritis. *Bye & Linares 12,900.*

GROSSULARIACEAE

Ribes leptanthum Gray — ***garambuyo***

This low shrub of the pinyon-juniper woodland produces sweet, purple fruits which are eaten raw. *Bye & Linares 12,924.*

LAMIACEAE

Mentha arvensis L. — ***yerba buena; poleo; menta;*** mint; brookmint

Mentha arvensis, native to the northern hemisphere, grows in moist areas of the valley floor and mountains and is especially common along irrigation ditches. The tea of this perennial herb is a common "remedio" for stomach ailments and colics. For treating fevers, it is used in a bath. The infusion is used as an eyewash. The herb is also used as a condiment for meat, soup and noodles. It is obtained from the field and is employed fresh as well as stored dried for future use. The plants from the mountains are said to be stronger than those from the valley floor. The dried herb is available in the pharmacies. *Bye & Linares 12,653; 12,279; 12,904; 12,907; 12,943.*

Monarda fistulosa L. var. *menthaefolia* (Graham) Fernald — ***orégano***

The annual herb *Monarda fistulosa* can be gathered on the valley floor and the lower, forested slopes of the mountains. A tea of this plant is drunk to alleviate stomachaches. Dried leaves are used as a condiment in beans cooked with garlic. Local plants are collected and sold in the pharmacies. *Bye & Linares 12,275.*

LILIACEAE

Asparagus officinalis L. ***espárrago***

The European perennial *Asparagus* is adventive in moist areas on the valley floor, especially near irrigation ditches. In June, the developing shoots are gathered, cooked and eaten as those of the cultivated form. *Bye & Linares 12,902.*

MALVACEAE

Malva neglecta Wallr. ***malvas***

This pink-flowered, prostrate species of *Malva* grows on the valley floor in gardens and cultivated fields. For sore throats and "orejones" (mumps), a poultice of the leaves and stems is placed on the painful area. *Bye & Linares 12,252; 12,906.*

Sphaeralcea coccinea (Pursh) Rydberg ***yerba de la negrita***

Sphaeralcea coccinea a common perennial weed in disturbed areas near fields and roadsides of the valley floor. The crushed leaves and orange-red flowers mixed with sugar are used to remove "grano enterrado" (hard boil). The crushed herb is also applied to infected cuts and cracked hands. It is not drunk as a tea. The hair is rinsed with an infusion of the plant in order to prevent it from falling out. Local, dried herbs are sold in pharmacies. The fresh plant is considered to be more effective than the dried herb. *Bye & Linares 12,249; 12,281; 12,905; 12,911; 12,950.*

ONAGRACEAE

Oenothera coronopifolia T. & G. ***hierba de San Juan***

An infusion of the leaves and flowers is used to wash infections, wounds and "granos enterrados." The white flowers of this perennial herb create showy patches in open areas of the valley floor and slope. *Bye & Linares 12,913.*

PINACEAE

Abies concolor (G. & G.) Lindl. ***pino real***

Abies concolor grows in the mixed conifer forests and along the lower slopes near the rivers. The wood of this tree is said to be of poor quality for cooking and heating. The ***trementina*** or resin that exudes from wounds on the trunks and branches can be applied to remove "granos enterrados." A poultice is made by combining the resin with ***yerba de la negrita*** *(Sphaeralcea coccinea).* For infections in the urinary tract, one teaspoon of the ***trementina*** mixed with sugar is drunk. *Bye & Linares 12,936.*

Pinus edulis Engelm. ***piñón***

As a major component of the pinyon-juniper woodland, *Pinus edulis* dominates the lower slopes of the mountains, especially on south-facing hill sides. The wood is valued along with that of ***pinabete*** *(Picea* sp.) for cooking and heating. The trees are said to yield more edible seeds when the spring is very rainy. *Bye & Linares 12,938.*

Pinus sp. ***trementina***

Resin from local pine trees (usually said to be ***piñón,*** *Pinus edulis)* is sold in the pharmacies. For treating hemorrhoids, it is mixed with powdered ***punchón*** *(Verbascum thapsus)* or ***punche mexicano*** *(Nicotiana* sp.), honey, and ***yerba mansa*** *(Anemopsis californica* (Nutt.) Hook. & Arn.) to form a ball which is administered as a suppository. Some ointment combinations also include ***romero*** *(Rosmarinus officinalis).* To remove silvers and boils, a mixture of ***trementina, punche,*** and honey is applied externally. *Bye & Linares 12,667.*

POLYGONACEAE
Eriogonum racemosum Nutt. ***cola de ratôn***

In open areas of pinyon-juniper woodlands, one can commonly find ***cola de raton*** along with *Seriphidium tridentatum*. *Bye & Linares 12,919.*

Rumex salicifolius Weinm. ***lengua de vaca***

Dried herbs (leaves and infructescences) from local sources in the mountains are available in the pharmacies. *Bye & Linares 12,664.*

ROSACEAE
Cercocarpus montanus Raf. ***palo duro***

This shrub is found in the pinyon-juniper woodland and into the conifer forests of the valley. *Bye & Linares 12,931.*

Rosa woodsii Lindl. ***champes; rosa de castilla***

The red fruits of wild rose (Fig. 2d), often found in the understory of the forests, are eagerly gathered in the fall. They may be eaten raw or mixed with apples and other fruits to make a jelly. Usually ***champes*** are not used alone because they impart a strong flavor. The flower petals are used in making a tea which is given to babies with colic. The tea is also used to alleviate "torzones de tripas" (intestinal cramps). Ground petals are used like talc to treat "chincual." *Bye & Linares 12,259.*

RUTACEAE
Citrus sp. ***hoja de limõn***

The leaves of this Asiatic tree grown in greenhouses and home windows are used in making a tea. Dried leaves are sold in the pharmacies. *Bye & Linares 12,658.*

SALICACEAE
Populus angustifolia James ***jara; álamo***

This distinctive tree in the valley, frequently found along streams and irrigation ditches, is used as living fences and windbreaks. The fresh twigs are chewed to fix loose teeth and to treat pyorrhea. The chewed leaves are said to help preserve the teeth because they have the action of chlorophyll. The leaves are also used to make a "remedio" for venereal disease (see *Urtica). Bye & Linares 12,250.*

Populus nigra L. var. *italica* DuRoi ***jara; álamo***

This introduced tree is used in the same manner as the preceding species. *Bye & Linares 12,251.*

URTICACEAE
Urtica dioica L. subsp. *gracilis* (Ait.) Selander ***ortiga; ortiguilla;*** nettle; poison-ivy

Urtica dioica grows in moist places on the valley floor and the lower forested slopes, especially along irrigation ditches and creeks. The leaves of this perennial, stinging herb are mixed with leaves of *Populus angustifolia* and fried onions *(Allium cepa* L.), boiled and drunk as a "remedio" for venereal disease. For those who have difficulty to "miar" (urinate), a tea of the herb is drunk. Locally collected plants are sold dry in the pharmacies. *Bye & Linares 12,264; 12,650; 12,942.*

OTHER PLANTS

Some plants were mentioned during discussions with Valley residents but were not available or in the appropriate state to make voucher specimens. These have been listed in alphabetical order by their local names. Probable identifications are given based upon descriptions and characteristics assigned to them or based upon field determinations of plant remnants. Future researchers should obtain specimens to better document these plants and their ethnobotanical data.

ajo y cebolla de la sierra

The bulbs and leaves of wild garlic and onion are said to be available all year round in the mountains. They are edible raw or cooked. Two species of *Allium* are reported from the basin of San Luis: *A. cernuum* Roth and *A. geyeri* S. Wats. (Dixon 1986).

alucema

The cultivated herb, *Lavandula officinalis* L., is obtained from the pharmacies *(Bye & Linares 12,266)*. Drops of the infusion are given to babies when they have "chincual."

altamisa

A very bitter herb, probably *Artemisia ludoviciana* Nutt. subsp. *redolens* (Gray) Keck, is known from the mountains ***(altamisa de la sierra)*** as well as from the valley ***(altamisa de la casa).*** A tea from foliage mixed with ***chamiso hediondo*** *(Seriphidium tridentatum)* is drunk by those with diabetes. It also is employed to alleviate stomach pains and colics and used in a bath for fever.

epazote; pazote

An annual herb referable to *Teloxys ambrosioides* (L.) Weber (syn., *Chenopodium ambrosioides* L.) is obtained dried in the pharmacies as well as from local and New Mexican sources *(Bye & Linares 12,271)*. It is used in preparing a tea, which can cause vomiting and nausea when consumed in quantity.

maravilla

The roots of a perennial herb, probably *Mirabilis multiflora* (Torr.) Gray and/or *M. oxybaphoides* Gray of the pinyon-juniper woodlands (Dixon 1986), are gathered locally and sold in the pharmacies. *Bye & Linares 12,274.*

pinabete

It refers to the spruces, *Picea engelmannii* (Parry) Engelm. and *P. pungens* Engelm.

punche mexicano; tabaco

This annual herb with large leaves, probably *Nicotiana rustica* L., is planted every summer. It may also refer to wild species of *Nicotiana*. In addition to smoking the prepared leaves, fresh leaves are crushed and inhaled to cure stuffy noses. It is also used in the preparation of a hemorrhoid remedy (see *Pinus)*, sometimes mixed with "cebo" (sheep fat), or added to ***punchôn*** to treat "dolor de ardo" (heat pain). The plant is used principally by the older people.

punchôn

This introduced biennial *(Verbascum thapsus* L.) is found in disturbed areas on the valley floor and mountain slopes. It is used in a hemorrhoid mixture with ***punche***

mexicano. It is available in the local pharmacies which obtain it from New Mexican sources *(Bye & Linares 12,654).*

quelite; lamb's quarters; wild spinach

Some weedy plants found on the valley floor and lower forested slopes are probably referable to both *Amaranthus retroflexus* L. and *Chenopodium berlandieri* Moq. ***Quelites*** are collected usually in June. Although no specimens were collected, the description from people in the valley suggest these plants. The young leaves of lamb's quarters are prepared and consumed as edible greens in the early spring (López T. 1975). This "wild spinach" is thought to be of special value in "building strength, keeping healthy" (Schulman and Smith 1962:69).

verdolagas; purslane

An annual weed, possibly *Portulaca oleracea* L., grows in disturbed areas of the valley. In the early summer (usually in July) the succulent leaves and stems are collected. They are eaten cooked and are valued for their blood-strengthening qualities (López T. 1975; Schulman and Smith 1962). The young stems and leaves can also be preserved using a canning process. Later, they are cooked with onion and garlic.

yerba del chivatito

A red herb, possibly *Teloxys graveolens* (Willd.) Weber *(Chenopodium graveolens* Willd.), is similar to ***lentejilla*** but much redder in color. It grows in moist sites between the lakes and foothills. A tea of the plant is said to be good for the heart, to treat "el frío en el estómago", and for children who wet the bed. The infusion used as a bath is a treatment for fevers and for scarlet fever in order to prevent the hair from falling out.

yerba del manso

This herb is said to grow in the mountains. *Anemopsis californica* (Nutt.) Hook. & Arn. (also known as ***yerba mansa***) is not reported in the Valley of San Luis but plants originating from northern New Mexico are available in the pharmacies *(Bye & Linares 12,645).* ***Yerba mansa*** is mixed with ***punche*** and applied as a suppository in the treatment of hemorrhoids.

yerba de la manzanita

This plant, possibly *Arctostaphylos uva-ursi* (L.) Spreng., is a popular medicinal herb, which is more common in New Mexico.

yerba del oso; yerba joso

A herb with red fruits, possibly *Heracleum sphondylium* L., grows on the slopes of the mountains. An infusion is used as a gargle for sore throats and as a tea for fever and rheumatism.

yerba de la sangre

A spreading plant referable to *Mahonia repens* (Lindl.) G. Don of the mountains has leaves which turn red in the fall. A tea is drunk to "limpiar la sangre" (clean the blood) and "engrosar la sangre" (thicken the blood) when one is anemic.

yerba de zorrillo

This annual herb, thought to be *Teloxys graveolens*, grows in New Mexico. A sun tea is made from the plant. It is sold in the pharmacies and originates from commercial suppliers *(Bye & Linares 12,272)*.

yerbanís; hierba anís

A dried herb ordered from Santa Fe, New Mexico, is used in flavoring the "biscochitos," the traditional sweet breads of "El Valle." ***Hierba anís*** available in Lujan's Place in Santa Fe is *Oligosporus dracunculus* (L.) Poljakov (syn., *Artemisia dracunculus* L.) *(Bye & Linares 12,690)* which is collected locally. It is reported from the basin of San Luis also (Dixon 1986). ***Anís*** *(Pimpinella anisum* L.) and ***anís estrella*** *(Illicium verum* L.) are also used as flavoring and are available in the local pharmacies. They are also popular "remedios" for alleviating stomachaches.

yerbabuena; yerba buena

An herb cultivated in the gardens and obtained in the pharmacies, probably *Mentha* X *piperita* L. and *Mentha spicata* L., is used in the same manner as *Mentha arvensis*. *Mentha spicata* is reported in moist sites on the valley floor (Dixon 1986).

DISCUSSION

The Valley of San Luis is a fertile ground for studying the ethnobotany of Hispano American plants. The concepts and employment of medicinal plants are particularly rich but changes are underway that threaten these customs with abandonment in the near future. Not only can one identify the movement and strength of the centralized powers of New Spain and later Mexico at its northern limit, but also the influence of Native Americans on the Hispanic settlers isolated for generations from their parent culture. An example of the hybridization of Hispanic and Indian customs have been suggested by the current use of medicinal plants such as Spanish herbs ***alhucema*** *(Lavandula officinalis)* and ***manzanilla*** *(Chamomilla recutita)* and such native roots as ***oshá*** *(Ligusticum porteri)* and ***inmortal*** *(Asclepias* sp.) (Appelt 1985; Moore 1977; Schulman and Smith 1962). These ideas are further developed for ***oshá*** below.

The dynamics of plant usage and concepts can be investigated by comparing the medicinal and edible herbs of "El Valle" with those of other parts of the US and Mexico. Although this is not the intent of the present discussion, preliminary examination suggests that the Hispanic settlers of the San Luis Valley share more with the residents of northern New Mexico (e.g., Santa Fe) than with those further south. To illustrate this pattern, one can consider the "chuchupate" complex (Linares and Bye in press). The Hispanic residents of the upper reaches of the Rio Grande (i.e., San Luis Valley and Santa Fe region) use the term ***oshá*** for *Ligusticum porteri* and recognize ***chuchupate*** as a synonym. Along the mid section of the Rio Grande (i.e., southern New Mexico) which was connected commercially with Chihuahua and Mexico City by a central route, ***chuchupate*** and ***hierba de cochino*** are applied to the same species. In the Lower Rio Grande, ***chuchupate*** with the similar uses refers to *Myroxylon balsamum* (L.) Harms, an aromatic stem of a tropical tree that is traded up the eastern route through Monterrey.

The prevalence of an indigenous common name for *Ligusticum porteri* and its use illustrate the importance of acquisition of a local resource and its subsequent modification in relative isolation. ***Oshá*** is probably derived from a term used by Native American Indians (Cobos 1983), possibly the Pueblo Indians of present-day northern New Mexico. The Tewa Indians applied the name ***'osa*** to *Ligusticum porteri* which was also known

to the Mexican traders and originally identified as *"Angelica"* (Robbins *et al.* 1916). ***Chuchupate*** is a term associated with New Spain (Falcôn 1777) and may be modified from a Nahuatl term such as chichipatli. The early Spanish settlers often modified "chi" to "chu" (see ***chuchumeca*** in Santamaría 1978) and "patli" to "pate". If ***chuchupate*** is based upon ***chichipatli,*** it would mean 'dog medicinal herb' from ***chi*** ('dog') and ***patli*** ('medicine', 'medicinal herb') (Simêon 1984). This is not unreasonable since many odoriferous plants are associated with terms dog, fox and coyote.

Two forms of ***oshá*** are recognized in northern New Mexico (Curtin 1965; Moore 1979) and in southern Colorado. One form is a thin, wild plant of the mountains (***oshá de la sierra***) and has white flowers. The other form is a robust, cultivated plant (***oshâ del jardin; oshâ del campo***) that has yellow flowers. The former is *L. porteri* and is said to be impossible to cultivate from seeds or from transplants in New Mexico (Moore 1979) and Colorado. The latter is reported to be *Angelica pinnata* S. Wats. (Curtin 1965; Moore 1979). Based upon Curtin's description and illustrations (1965: p. 139; figs. 9 and 19), the yellow-flowered ***oshá*** is *Levisticum officinale* (lovage) given the flower color, the ternately compound leaves with cuneate leaflets with coarsely dentate margins, the conspicuous involucre of narrow reflexed bracts, and the densely branched stems *(Angelica* usually has white flowers, pinnately compound leaves with ovate to lanceolate leaflets with serrate margins, involucral bracts lacking, and diffusely branched stems). In northern New Mexico, lovage is cultivated and known as an escape (Martin and Hutchins 1981). It is considered to be a native of southern Europe and is valued for its aromatic fruits (Bailey and Bailey 1976).

The role of ***oshâ*** in the Hispano American culture of southern Colorado as well as in adjacent New Mexico appears to have been strongly influenced by Native Americans. The use of its Pueblo Indian name rather than a Hispanisized central Mexican name that was accepted in New Spain and that name's application to an introduced European cultivated herb *(Levisticum)* argue that *Ligusticum porteri* was adopted by the early Spanish and Mexican settlers of New Mexico and Colorado from the local Indians. Its employment today reflects the influence of both Native American customs (e.g., cough and gastrointenstinal medicine) as well as Hispanic concepts (e.g., protection from evils of "brujos" and "enconadores").

The residents of "El Valle" are aware of the differences between wild plants (those from the mountains or "de la sierra") and anthropogenic plants (those from fields, yards, roadsides etc. or "del campo" and "de la casa"; and those cultivated in the garden or "del jardîn"). In some cases, the plants are different species as in the ***oshâ*** discussed above or ***jara*** *(Populus angustifolia* and *P. nigra).* In other cases, the plant may be the same species such as ***plumajillo*** *(Achillea lanulosa)* or ***poleo*** *(Mentha arvensis).* It is a general rule that the plants from the mountains are considered more effective than those from other places. Different species of herbs are recognized as being related. In the case of *Achillea lanulosa* and *Artemisia frigida,* both share the name ***plumajillo de la sierra*** and are employed for gastrointestinal ailments.

The herb collectors are also aware that the season of gathering medicinal roots is important. The roots (e.g., ***inmortal*** and ***oshâ***) are gathered in the fall after the growing season, when the above-ground herbage has died back and the roots are large and solid with stored substances. Collection in the late spring and summer is avoided when the roots have exhausted their storage products from the previous season. Although not a "remedio" of the San Luis Valley, the perennial herb *Conium* (a member of the same family as *Ligusticum),* has been shown to use alkaloids (a secondary metabolite) like re-cycling co-enzymes in the fruiting process (Fairbairn 1971). Therefore certain biodynamically active principles would not be present in the same concentration in the root prior to or at the time of flowering as after the growing season. In order to harvest the root at the time of peak effectiveness, one would need to gather it after the substances

had been replenished and stored in the root in preparation for the following season and not during the period of mobilization and utilization of the substances in the natural plant life cycle.

Concepts and names of plants native to "El Valle" often have Spanish roots. ***Garambuyo*** is applied to a wild spiny gooseberry *(Ribes leptanthum)* with a small, sweet, many-seeded, purple-colored fruit. Today in Mexico, ***garambullo*** is most frequently applied to the small, sweet, many-seeded, purple-colored fruit of the cactus, *Myrtillocactus geometrizans* (Mart.) Console. In New Mexico, ***garambullo*** is used for gooseberry (Cobos 1983). The Spanish terms ***poleo*** and ***menta*** are applied to native as well as introduced species of *Mentha*. These cases suggest that structural and chemical (e.g., odor and flavor) characteristics of local plants are similar to those of Mexican and European plants with which their ancestors were familiar.

The pharmacies play an important role in selling certain "remedios" to those who can not have access to the plants in the fields or mountains or who do not know of local sources. Of the 35 taxa obtained from the pharmacies, 13 (ca. 37%) were collected locally. Of the 22 species purchased from out of state, only 2 (ca. 6% of the total sample) were also supplied from local sources. They are *Achillea* and *Ligusticum*. Of the local plants, all occur as weeds on the valley floor and slopes except *Citrus* (which is cultivated in greenhouses) and *Ligusticum*. The latter plant is of interest because of the great demand yet limited distribution and declining abundance in the mountains. To date, ***oshâ de la sierra*** has not been cultivated successfully. The scarcity of this root may be an important factor for importing it from Arizona suppliers and hence the introduction of the synonym, ***chuchupate,*** under which it is sold.

It should be noted that the pharmacies, like general stores, sell a variety of goods. Herbs are sold as a convenience to the customers and not as prescription medicine. The customers know the uses of the "remedios" and hence the pharmacies play an important role in maintaining part of the cultural heritage of "El Valle."

Consequently, our present-day ethnobotanical knowledge of the Spanish Americans in "El Valle" does not indicate a simple adoption of local customs nor the dominance of introduced traditions from the south. The mixing of Native American and Hispanic ideas about plants was mediated by a selective diffusion of local and exotic knowledge in an isolated cultural and natural environment.

With today's rapid acculturation of the Hispanic people in "El Valle," the loss of several generations of ethnobotanical knowledge and experience is imminent. Fewer Hispano American young people learn about and use traditional plants. Many are leaving "El Valle" in search of employment (López T. 1975). Middle-aged people once may have used or heard of certain plants but no longer employ them. This situation parallels the pattern of the older people speaking mostly or only archaic (16th and 17th century) Spanish while the children are unable to communicate in their mother tongue. Local citizens, students and teachers recognize the urgent need to rescue their ethnobotanical traditions from further erosion due to Americanization. "Foreign" ethnobotanists (e.g., those from Colorado, New Mexico and Arizona) are needed to assist them in documenting their botanical knowledge.

CONCLUSIONS

A brief study of the plants used traditionally by the Hispano Americans in south-central Colorado recorded 57 taxa. About 75% (n = 43) are native plants while about 25% (n = 14) are introduced. In addition to gathering these herbs in the mountains, fields, and gardens, the local pharmacies sell 22 species obtained from commercial suppliers. About 41% (n = 9) of the plants are found in the region. The employment of medicinal plants suggests that the Native American Indian culture has had as strong an influence on Hispanic peoples as the Mexican Spanish culture.

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It is a wonderful thing that not just ethnobotany is in this work; too narrow a focus would have spoiled the richly detailed picture Felger and Moser have had the privilege to record. As it is, *People of the Desert and Sea* is a pleasure to read and a treasure for the library.

As the reader pours over the long section which takes up each of the 400-plus plants recognized by the Seri, the value of having a linguist and an ethnobotanist working together is appreciated in a personal way. The plant names are given in the binomial standard of the dominant culture, in Seri, in translation from Seri to English, both gloss and free translation. As you read them all, a sense comes of how the Seri evaluate the things of their world, and this is not an interpretation by ethnographers, but as close as one can come through words to directly seeing the thought of other human beings.

The volume's contents cover virtually everything most ethnographies have to offer: material culture, religion, food and water gathering techniques, recreation, music, world view, relation to other groups. In spite of the fact that this is more than expected for an ethnobotanical treatise, the reader will become so interested that he will want more. For instance, the methodology of research is hinted at in many places, but one comes to want a full explanation of how informants were acquired, who they were over time, whether there were disagreements over data and how these were resolved.

Appendix B, *Seri People Named in the Text* is confusing and tantalizing, has loose ends and hints at more good stories and incidents yet untold. It sparks another desire: do give us a genealogical chart for at least the Astorga family line. And if it is known, describe how prominent family lines or individuals figure in this chiefless culture, before cash economy and now.

When researchers have spent twenty and thirty years patiently teasing out information about a people struggling with the pressures of corrosive change, one might expect the tale when told to be grim or boring, or both. These authors give us a non-judgemental account that is meticulously precise, lays out uncertainties and contradictions with no excuses, and is neither dry nor maudlin. The style seems to reflect the subject, people described to be ". . . gregarious, outgoing and aggressive . . . sharp sense of humor . . . highly independent, nonconforming, and quick to adjust for the sake of convenience."

There are some 400 drawings, maps, and superb black and white photographs which probably justify the cost of the volume in themselves. The photos span many years of Seri contact with cameras; whoever did the printing is very competent. Many of the plants were photographed by Felger, who has an intense sense of composition and an eye for capturing the important detail. Almost, the reader forgets there are no color shots.

The plant descriptions themselves include drawings or photos of those species particularly important to the Seris, and of the processing systems that rendered them available for use. For instance, the various shapes of cactus fruit pickers, the way they are made and employed are shown in detail. A good sense of the labor and practiced skill required to gather wild resources is obtained from photographs of the process involved and the total take in possession of the successful gatherer, plus records of the person-hours required per quantity obtained. One example among many is the account for roots of *Cnidoscolus palmeri*. Anyone who has tried to estimate work-weeks or energy budgets or who has tried to recreate process in resource exploitation from fragmentary ethnographic accounts will be more than pleased with the coverage here.

The authors have tried to encompass everything the Seri recognized as a distinct plant, a folk taxonomic group of whatever sort. Some species important in other regions were not used here, often because they lacked quantity or reliability in Seri territories. Some species are only discerned, not used, which gives one a feel for the spaciousness of the Seri lifeway—hard but not brutish; there was time and attention given to close observation of all things, not just the utilitarian ones.

Physically, the volume is sturdily bound and the paper seems to be designed for use and reuse. The print and format are easily read, and there is adequate space for marginal notation. Happily, there are few typographical errors and the main index works; these are not things one may take for granted anymore in published material.

Cross-referencing can be uneven. Occasionally one gets part of an interesting story and no clue where to look for the rest of it. A case in point is reference to supernatural things concerning "the story of Lola Casanova and Coyote Iguana". Forty-two pages later, if you know where to look, you can find out what this is all about. Similarly, there is the displacement of details on how certain games are actually played from the section on Games to the section on Plant Species Descriptions. Even though the reader is referred to certain species, e.g. *Phragmites*, one does not expect to find more detail on game rules in the plant section than is present in the games section.

There are two assertions about the Seri which do not seem solidly supported by the text. They are subjects of considerable interest. First, the authors seem to want to say that the Seri are still intact as a cultural entity. "Although much traditional knowledge vanished with the death of each older person, Seri cultural identity remained strong in 1983. Their language was intact and their population increasing. The Seri were meeting the challenges of the modern world in much the same way as they reacted to earlier Spanish-Mexican influences—by taking advantage of new resources, new markets, and new opportunities without radically altering their loose and flexible way of life. They were accepting what they could of the new culture and rejecting that part they did not want."

Yet much of what is described in this book is gone or rapidly going, e.g. turtle hunting and usage, balsa boat building and usage, the everyday items of material culture such as utensils, houses, clothing. Food comes from stores in the face of land and sea resources depleted. To get the cash for the new things, the fashioning of wood carvings and basketry, of items largely designed for outside sale, has become important instead of food gathering and preparation. Water is obtained from wells and faucets, a fact which changes the pattern of land usage and the dispersal of social units. Their religion has been replaced by Christianity, and that alters social structure too. Genetically, they are changing with marriages into the blood lines of other peoples. It is hard to understand, at least if one has not been with them recently, how they have *not* been altered. If one took pictures of Seri in the mid-1980's, concentrating on how most people now live, what would one see? Or is what one sees less important than the living language, even with the Spanish additions? Reading these pages provokes serious thought about cultural identity and the ways it may be recognized, or maintained.

Is there such a thing as a legally recognized Seri Indian territory or reservations? Is the traditional land reserved in any way for them as a cultural, racial entity?

The second assertion is about psychotropic plant usage. The authors say the Seri do not use psychoactive materials. Nevertheless, the text relates that the Seri have their tobaccos and their cactus fruit wines. If they make use of any experiences derived therefrom, the connection is not clear in this work. Then the smoking of *Ruellia californica* leaves or flowers in a clay pipe is mentioned. This plant is known, like *Datura*, to "make people crazy." Obviously, some people used it anyway, maybe just for fun, but since there are references to Seri receiving powerful information in dreams, and there is the tradition of going on vision quest for power, the setting in which psychotropic plants could have been utilized did exist. If they make little of this usage to outsiders it may be because it is a matter of small moment, now at least, or it may be that ways of acquiring power are considered quite valuable and not mouthed about. Here again, it would help to know what the investigator-informant relationships were.

Quibbles and questions will arise, unique to the reader and his or her interests, because this account of the Seri is full and graphic and lively.

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COW-PARSNIP *(HERACLEUM LANATUM* MICHX.): AN INDIGENOUS VEGETABLE OF NATIVE PEOPLE OF NORTHWESTERN NORTH AMERICA

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ABSTRACT.—Cow-parsnip *(Heracleum lanatum* Michx.), a green vegetable used universally by native people in western North America, is especially fascinating when considering its nutrient potential in native diets, its toxicity, and processing techniques developed by native people to transform the plant to a food by reducing its toxic components. Botanical characteristics, extent of use by native Indian groups, decline in use by the Nuxalk people during this century, and the contents of nutrients and furanocoumarins of the plant are reported.

INTRODUCTION

At least seven species of *Heracleum* have been reported to be edible (Hedrick 1972). The common North American cow-parsnip, *H. lanatum* Michx., is no exception, having been used as a green vegetable by native peoples throughout much of its range (Chesnut 1902; Yanovsky 1936; Hellson and Gadd 1974; Turner 1975, 1978; Heller 1976; Kari 1977; Lamont 1977; Turner *et al.* 1980; 'Ksan, People of 1980; Norton 1981; Arnason *et al.* 1981). In northwestern North America it was one of the most important of all indigenous green vegetables, in terms of the cultural and geographical extent of its use and the quantities consumed.

Heracleum species, in common with certain other plants in the same family (e.g., garden parsnip), contain furanocoumarins, compounds which, in the presence of light of 350-370 nm (i.e., the ultraviolet component of ordinary sunlight) form complexes with thymine residues of DNA in human epidermis when the plant is handled. This phototoxic effect results in blistering and hyperpigmentation of the skin (Camm *et al.* 1976; Mitchell and Rook 1979; Scheel 1973). Native people were certainly aware of the irritant properties of *Heracleum lanatum*, as shown by the caution with which they gathered and prepared it for use. Only the young leafstalks and budstalks were used as greens, and these were always carefully peeled before being eaten (Chesnut 1902; Lamont 1977; Kari 1977; Hart 1979; Turner *et al.* 1980; Norton 1981; Turner *et al.* 1983).

Despite the widespread use of cow-parsnip as food, its nutritional contribution to the traditional native diet and its potential toxicity have received little attention in published literature. Biochemical studies relating to its furanocoumarin content (Camm *et al.* 1976; Mitchell and Rook 1979) have concentrated on the plant as a whole, rather

than on its younger growth stages or edible portions as native people traditionally prepare them. The goals of this study were to provide a botanical description of *Heracleum lanatum*, summarize its past and present use as food in northwestern North America, assess its nutritional contribution and discuss the effect of its phototoxins in determining traditional preparation techniques.

BOTANICAL DESCRIPTION

Heracleum lanatum Michx., (Fig. 1) the common cow-parsnip, a robust perennial herb of the celery or umbel family (Apiaceae, or Umbelliferae), is the only one of 60 species of *Heracleum* native to North America. Growing from a stout taproot or cluster of fibrous roots, it can attain a height of 3 m at maturity but averages 1.5-2.0 m. The leaves are compound, in three segments, with broad, petiolate, coarsely toothed and palmately lobed leaflets usually 10-30 cm long. The terminal leaflet is usually as wide as it is long; the two lateral ones are narrower and often asymmetrical. The stems and lower leaf surfaces are usually sparsely to densely hairy. The leaf petioles are sheathing and conspicuously expanded. The inflorescence is large, of compound umbels on terminal and auxillary peduncles. The terminal umbel can exceed 20 cm across. The flowers are small and white. Flowering time is from June to August, depending on latitude and elevation. The dry, whitish fruits are oval, flattened, and winged (Hitchcock *et al.* 1969).

FIG. 1.—Cow-parsnip *(Heracleum lanatum)*, in flower.

The entire plant, especially when mature, has a strong, pungent odor, belying the mild, sweet flavor and aroma of the young peeled stalks.

Cow-parsnip grows, often in dense patches, along streambanks, and roadsides and in moist meadows, thickets, and clearings from sea level to subalpine habitats. It ranges from Alaska to Newfoundland, southwards to California, Arizona and Georgia. It also occurs in Siberia and the Kurile Islands (Hitchcock *et al.* 1969, Pt. 3:535), and the Aleutians. Hulten (1968) provides a distribution map.

EXTENT OF USE BY NATIVE INDIAN GROUPS

Cow-parsnip was enjoyed by virtually all native people throughout northwestern North America (Table 1). Today, it is often called "wild, or Indian rhubarb" ('Ksan, People 1980), or "Indian celery" (Norton 1981). It was gathered in spring, from early April to late June, depending on the area, but always before flowering had occurred (Fig. 2). Once the budding inflorescence, enclosed within a sheathing leafstalk, began to swell and elongate, the plant was no longer considered suitable for eating. It becomes "bitter, tough and no good" ('Ksan, People of 1980). Some native people have noted that plants growing in the shade remain at the edible stage longer into the season than those growing in the sun, and are also more tender (Turner *et al.* 1976, 1983).

Since the spring season was the only time that cow-parsnip and other green vegetables (including salmonberry and thimbleberry sprouts, fireweed shoots, and horsetail shoots) were available, they were much sought after and gathered in considerable quantities during this time. Gorman (1896), for example, notes that cow-parsnip was " . . . preeminently the fresh vegetable of the [Alaskan] natives, who gather and eat the succulent leaf-stalks and young stems in enormous quantities, the ***Kloochmen*** [Chinook jargon for women] and children gathering it most of the day when it is in the proper condition . . ." People often traveled long distances (50 miles or more) to get cow-parsnip shoots. For example, Sechelt people made special trips to Narrows Arm, Vancouver Bay, and many other places (Bouchard 1977). Within the last century some Okanagan-Colville families made the

Table 1.—Indian groups in British Columbia and Neighboring Areas Known to have eaten *Heracleum lanatum*.

Indian group (dialect) (Language Family)	Native name[a]	Reference
Tanaina (all dialects) (Athapaskan)	***ggis***	Kari, 1977:83
Tlingit (Athapaskan)	***yon-a-eth***	Gorman, 1896:76
Haida (Kaigani) (Haida)	***hlk'iit***	Norton, 1981:444
Haida (Masset, Skidegate) (Haida)	***ɬkit***	Turner and Levine, 1972a: 10, 1972b:10
Coast Tsimshian (Tsimshian)	? ***peyintz***	Gorman, 1896:76
Nass-Gitksan (Nisgha) (Tsimshian)	***hamook***	McNeary, 1974:40

Table 1.—Indian groups in British Columbia and Neighboring Areas Known to have eaten *Heracleum lanatum*. (continued)

Indian group (dialect) (Language Family)	Native name[a]	Reference
Nass-Gitksan (Gitksan) (Tsimshian)	***gatl'okwots*** (another name also exists)	People of 'Ksan, 1980:86
Nuxalk/Bella Coola (Salish)	***xwik'***	Turner, 1973:201; Nuxalk Food and Nutrition Program, 1984:36
Southern Kwakiutl (Wakashan)	ls: ***gistem*** bs: ***k'uḿaki'i***	Turner and Bell, 1973:276
Nootka (Hesquiat) (Wakashan)	ls: ***qiɬcu·p*** bs: ***huma·q*** pl: ***qiɬcmapt***	Turner and Efrat, 1982:60
Nootka (Manhousat) (Wakashan)	ls: ***kilhtsuup*** bs: ***huḿaak***	Turner, Ellis and Bouchard, 1976:7
Nitinaht (Wakashan)	ls: ***quistu·p*** bs: ***hu·ba·q*** pl: ***quistu·papt***	Turner et al., 1983:91
Makah (Wakashan)	***kīstop***	Gunther, 1973:42
Comox (Mainland) (Salish)	ls: ***ɬtútamishúḿa*** bs: ***mén7úḿa*** pl: ***xákwu***	Bouchard, 1973:7; Bouchard, 1975:1,2
Sechelt (Salish)	***yálup***	Turner et al., 1972b:8
Halkomelem (Cowichan) (Salish)	***sa·q́^{w}***	Turner and Bell, 1971:89
Halkomelem (Upriver) (Salish)	"sprout": ***sóqw'*** pl: ***yó:lé, yóle***	Galloway, 1979:6
Straits (Saanich) (Salish)	***sa·q́^{w}*** ("not peppery"); OR ***yóle7*** ("peppery")	Turner and Bell, 1971:89; Turner and Bouchard, 1972:6
Clallam (Salish)	***sx^{w}mek^{w}usnen [s(u)mkwoosung]***	Fleischer, 1980:209; Turner, 1974:8
Squamish (Salish)	***yúla7***	Bouchard and Turner, 1976:65
Quinault (Salish)	***waká*** ("kills the pain")	Gunther, 1973:42

Table 1.—Indian groups in British Columbia and Neighboring Areas Known to have eaten *Heracleum lanatum*. (continued)

Indian group (dialect) (Language Family)	Native name[a]	Reference
Quileute (Chimakuan)	***tl'ó·pit***	Gunther, 1973:52
Lillooet (Pemberton) (Salish)	ls: ***nk̲ayxwxn*** ('man-foot') bs: ***yâk̲saxn*** ('woman-foot') pl: ***hâkwa7***	Turner et al., 1972a:12
Lillooet (Fraser River) (Salish)	ls, pl: same as above; bs: ***nmulhatsxn*** ('woman-foot')	Turner et al., 1972a:12
Thompson (Salish)	***hēk^{w}u?***	Turner et al., 1984
Shuswap (Salish)	***x̲wtalhp***	Palmer, 1975:56
Okanagan-Colville (Salish)	***x̲wux̲wtilhp***	Turner et al., 1980:62
Flathead-Kalispel	***hoxtâɬp; x̲wte***	Teit, 1973:344; Hart, 1979: 293
Coeur d'Alene (Salish)	***x̣ôx̣ɬp***	Teit, 1973:91
Chilcotin (Athapaskan)	***sol***	Tyhurst, pers. comm., 1975.
Carrier (Athapaskan)	***gwas***	Carrier Linguistic Committee, 1973:82
Slave (Athapaskan)	no name given, but note of use	Honigmann, 1946:35
Kootenay (Kootenay)	***wum?at***	Hart et al., 1981:46
Stoney (Algonkian)	***yazobi***	Scott-Brown, 1977:71
Blackfoot (Algonkian)	***po-kint-somo***	Johnston, 1970:316

[a]Orthographies used are those given in original references. Note: ls = leafstalks; bs = budstalks; pl = whole plant; unless otherwise indicated, name refers to whole plant.

FIG. 2.—Cow-parsnip at young (pre-flowering) stage, when stalks are harvested for eating.

gathering of cow-parsnip an occasion for Sunday picnics (Turner *et al.* 1980). Some native people have noted, and the authors have also observed, that certain cow-parsnip populations yield better-tasting shoots than others. Formerly, people had special patches where they went each year to get the best shoots (Turner and Efrat 1982; McNeary 1974). Furthermore, some people felt that the local plants "need to be picked, as it is remembered that when the plants were regularly harvested they grew much taller" (Norton 1981).

Almost all native people distinguished between the leafstalks (petioles) and inflorescence bud stems of cow-parsnip when gathering and eating the plants. Although both were considered edible, they were treated almost as two different foods. Usually the distinction was marked linguistically, the two types being called by different, linguistically unrelated names (Table 1). Some groups such as Lillooet, Thompson and Mainland Comox, made a "sexual" differentiation: the petioles were associated with females [e.g., in Lillooet, "woman stalks" (lit. 'woman-foot')], and the budstems were associated with males [cf. Lillooet, "man stalks" (lit. 'man-foot')(Turner *et al.* 1972a)]. This type of differentiation is also reported for more eastern groups, such as Blackfoot (Johnston 1970), Stoney (Scott-Brown 1977), and Fisherman Lake Slave (Lamont 1977). The budstalks are sometimes described as having a "lump" on the side (referring to the inflorescence bud encased in its leaf sheath) (Bouchard 1975).

Native people invariably warn one that if all the outer skin (epidermis plus fibrous tissue underlying the epidermal cells) is not removed, it will cause one to get an "itchy mouth" (Bouchard 1977), or sores and discoloration of the skin around the mouth (Turner *et al.* 1976; Turner 1971; Turner *et al.* 1983), or "burned" mouth and skin (Kari 1977; Norton 1981). A Manhousat man noted that he always stuck the stalks with a stick before gathering them (Turner *et al.* 1976). Apparently this was to release the irritant particles from the outer skin of the plant. The authors, on following this procedure, noticed a fine cloud of dust-like particles fell from the leaves and stalks when the plants were struck. Further investigation would be required to determine if this dust does indeed contain high concentrations of furanocoumarins.

Methods of peeling were different for petioles and budstalks. Petioles were often split longitudinally with the thumbnail, and the inner edible portion pulled from the fibrous outer skin by bending it backwards near the bottom until it broke, then grasping the broken end and pulling it downwards. Sometimes, the fibrous part was held in the teeth while this operation was carried out, but care was taken never to let it touch the lips (Turner *et al.* 1972a). The main inflorescence stalks, which are cylindrical with a large central hollow space, were broken off and peeled by separating the skin from the edible portion at the bottom and pulling it upwards in several pieces (Fig. 3). Sometimes the stems were twisted as they were peeled, facilitating the removal of the skin (Turner *et al.* 1976).

After peeling cow-parsnip was usually eaten raw (Turner 1971; Turner and Bell 1971; Turner *et al.* 1972b; Turner and Bell 1973; Turner *et al.* 1976; Turner and Efrat 1982; Bouchard 1973; Galloway 1979; Kari 1977; Heller 1976; 'Ksan, People of 1980; and R. Tyhurst, pers. comm., unpublished notes on Chilcotin plant names). Sometimes, as noted by Morice (1893), the stalks were first heated in the fire before being peeled: "It [cow-parsnip] is often used while fresh and unprepared save by the stripping of its fibrous envelope. But if a fire is at hand, a Carrier [Indian] will generally treat it to a slight roasting through the flames previously to peeling off the stalk" (Morice 1893). Other groups that sometimes roasted the stalks included the Gitksan, Kootenay, Blackfoot, and Stoney ('Ksan, People of 1980; Hart *et al.* 1981; Johnston 1970; Scott-Brown 1977). The stalks could also be cooked by boiling (Turner and Bell 1971; Turner *et al.* 1972a; Kari 1977; 'Ksan, People of 1980). Kari (1977) notes that the Tanaina chopped, then cooked them in stew or soup. Traditionally, the stalks, like many vegetable foods, were served with a dressing of seal or whale oil (Gunther 1973; Turner *et al.* 1983; Heller 1976) or ooligan (eulachon) grease (Turner 1971, 1973; Kari 1977; Nuxalk Food and Nutrition Program Staff 1984). More recently, the stalks have been dipped in sugar or honey, with or without the oil or grease (Turner 1971; Turner *et al.* 1983; McNeary 1974). Some people stew them with sugar to make a dessert (Scott-Brown 1977). Others serve them with fried onions and salt pork ('Ksan, People of 1980).

FIG. 3.—Cow-parsnip budstalks, partially peeled.

Although cow-parsnip stalks were usually eaten fresh, at least some groups also stored them for later use. The Kaigani Haida, for example, picked and peeled the stems, then packed them away in grease for winter. Norton (1981) notes that they were "stored in quantity." The Gitksan dried the stems for storage: "Wild rhubarb was split into strips before the end of June and air dried in the sun. 'We used to store it away in boxes for winter or just left it in bundles in our house. I can remember seeing it hanging over fences and verandah rails to dry.' " ('Ksan, People of 1980). Some contemporary Lillooet people freeze the peeled stalks for storage (Turner *et al.* 1972a). Today some people of the Pemberton Lillooet in Mount Currie, B.C., cut the stalks in thin strips, in the manner of French-cut green beans, then blanch and freeze them, to be used later in soups and stews.

Among the Nootka and Nitinaht people, pregnant women were warned not to eat the flower bud stalks. One Nitinaht man stated that to do so would cause the woman's child to become epileptic (Turner *et al.* 1983). A Manhousat man said that it would cause the baby, when it was born, to lose its breath when it cried and choke (Turner *et al.* 1976). A Hesquiat woman stated that the Nootka name for the bud stalks, "humaak", is related to the term, "humakshitl", for "when a baby cries so much that it almost loses its breath and chokes." When a child cried this hard, the elders at Hesquiat would say, "It must have been eating humaak." (A. Paul, pers. comm., to R. Bouchard, pers. comm., 1982).

OTHER USES OF COW-PARSNIP

Several miscellaneous uses of cow-parsnip have been reported. The Sechelt saved the fruits of cow-parsnip to use as flavoring in winter cooking (Bouchard 1977). Chesnut (1902) was told that the native people of Round Valley, California used the basal portion of cow-parsnip as a substitute for salt. It was dried in short cylinders and eaten either in a dry state with other food or cooked with foods to be flavored. Quileute and Makah girls used to make baskets from the large mature inflorescent umbels, by twining the rays with seaweed. They filled the basket with shells for playing (Gunther 1973). Nitinhat children sometimes made toy animals with the swollen leaf sheaths being used as the body (Turner *et al.* 1983). Cow-parsnip had a wide variety of medicinal uses, many of which are listed by Moerman (1977). At least seven native groups in North America used the plant as a dermatological aid, despite its known irritant properties.

EXTENT OF USE OF COW-PARSNIP BY THE NUXALK PEOPLE

The extent of use of cow-parsnip was examined in detail by an interview study of three generations of women of the Nuxalk Nation of Bella Coola, British Columbia. Data on use and taste appreciation of this vegetable were collected along with that of 70 species of traditional foods during 1983 (Kuhnlein 1986). The women interviewed were 61 in number, 20 with birthdates for 1904-30, 20 with birthdates 1931-50, and 21 with birthdates from 1951-63. They were asked to recall their use of cow-parsnip during two (younger women) or three (older women) decades of their lifespan, and to assign a taste appreciation score.

In all, 45 of the 61 women reported using cow-parsnip as food. Since the 1920s there has been a steady decline in use of cow-parsnip (Fig. 4), and this vegetable has been used only fresh in season by Nuxalk women, (ie. it has never been preserved for later use). At this time, use of cow-parsnip is only occasional. It is only rarely harvested and prepared for family mealtimes, being used instead as a "treat" when walking through the village or country side.

Taste scores on a 5-point hedonic scale for this vegetable are given in Table 2. Generally, it is considered from "fair to good", with an overall mean score of 3.6 out

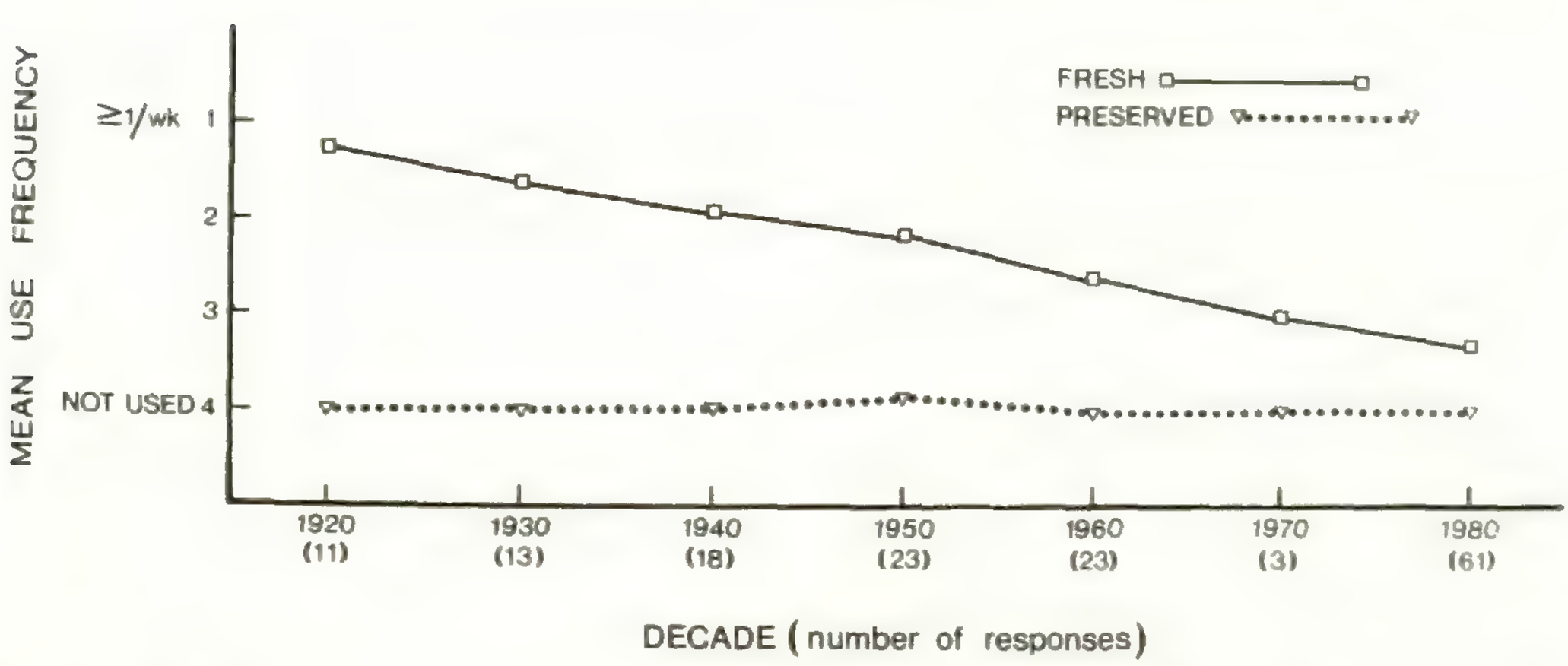

FIG. 4.—Nuxalk use of cow-parsnip *(Heracleum lanatum)* from 1920-1980. Use frequency scores were: 1—used at least once per week in season, 2—used less than once per week, but more often than once per month, 3—used only occasionally, and 4—not used. Number in ( ) below the decade is the number (n) of women recalling use at this time. Points on the graph are mean scores for n women.

Table 2.—Taste scores* for cow-parsnip *(Heracleum lanatum)* by Nuxalk women.

Birthdate	No.	Mean Score + S.D.
1904-30	17	3.8 ± 0.5**
1931-50	15	3.6 ± 1.0
1951-63	13	3.2 ± 0.9**
	Overall Mean	3.6

*Hedonic taste appreciation scale:
5 = Best; no improvement possible
4 = Good; enjoy it
3 = Fair; could eat it, it's OK
2 = Poor; edible, but that's all
1 = Terrible; inedible, definitely don't like it
0 = Inedible

**Significantly different with Student's t-test at p = 0.04

of 5. Scores given by the older women were significantly higher (p = 0.04) at 3.8 ±0.5 than those of the youngest women at 3.2 ±0.9 with middle-aged women have intermediate scores at 3.6 ±1.0.

It can be concluded that this vegetable is used less often today than in earlier decades of this century, and that in general, older women like it and use it more often than younger women.

FIG. 5.—Cow-parsnip stalks, collected for nutrient analysis, before peeling.

NUTRIENT CONTENT OF COW-PARSNIP

Samples of cow-parsnip were harvested from the Victoria area of Vancouver Island and from reserved lands of the Nuxalk Nation in Bella Coola, B.C. Both the leafstalks and inflorescence bud stems were harvested using scissors (Fig. 5). The stems were peeled with the aid of sharp, stainless steel knife and frozen in plastic bags until analysis.

A profile of nutrient contents of *Heracleum lanatum* is given in Table 3. In general, this vegetable is similar in composition to other green vegetables, such as celery, as reflected in a moisture value of more than 90%, and lipids, ash and protein less than 1.0%. As one of the first fresh plant foods of the spring in traditional diets, cow-parsnip would be likely to provide meaningful amounts of folate and ascorbate in the total diet of coastal peoples who had winter diets of dried fish, preserved berries and roots, and marine fats. Levels of vitamin A as carotene are low (7-8 I.U.), so that this food would not have been a major source unless eaten with ooligan grease, which has a great deal of retinol, a primary form of vitamin A (Kuhnlein *et al.* 1982).

There are a few published values for nutrients in *Heracleum lanatum* from other parts of North America. As nearly as can be determined, none of the authors reported values on the peeled stalks, but rather analysed the complete stalk. Yanovsky and Kingsbury (1938) reported a Utah sample to contain 90.1% water, 1.6% protein, 1.5% ash and 0.8% hemicellulose. Berberie (1946) reported *Heracleum lanatum* of New Brunswick to contain 0.113 mg riboflavin per 100 g samples using the microbiological assay of that era. The ascorbate value for one sample of cow-parsnip reported by Norton *et al.* (1984) was 60 mg/100 g whole stalk. This is quite high in comparison to our findings, but might be explained by differences in sampling, storage, geography or method of analysis.

Mineral levels in this food are modest, but would have been significant in the diet if eaten in reasonable amounts in spring when other plant sources of minerals (for example, magnesium, calcium, iron, zinc and copper) may have been lacking. Although

cow-parsnip would not have contributed much energy, protein, carbohydrate or fat in the typical coastal diet, its primary nutritional value as a spring food would have been in its contribution of the so-called "water soluble" vitamins. In addition, its value as a food would be enhanced by its pleasing texture and flavor, especially at this time of year.

Table 3.—Nutrients in raw, peeled stalks of cow-parsnip *(Heracleum lanatum)*.

Nutrient per 100 g fresh weight	N	Mean level	Range
Moisture, g	5	95.02	93.2 - 96.0
Total lipids, g	4	0.24	0.13 - 0.35
Ash, g	5	0.57	0.38 - 0.59
Protein, g	3	0.40	0.20 - 0.65
Total Carbohydrate, g	4	3.77	3.11 - 5.66
Energy (approx.), kcal		20	
B-carotene, I.U.	2	7.5	7 - 8
Folacin, free, ug	2	13.4	10.2 - 16.9
total, ug	2	16.1	11.8 - 14.3
Ascorbate, mg	4	3.5	2.5 - 4.3
Calcium, mg	4	27.7	15.4 - 37.4
Phosphorus, mg	4	18.5	13.7 - 23.7
Strontium, mg	4	0.19	.18 - .20
Iron, mg	4	0.30	.23 - .46
Copper, mg	4	0.36	.02 - 1.1
Zinc, mg	4	0.38	.13 - 1.0
Magnesium, mg	4	11.7	9.5 - 15.5
Manganese, ug	4	87.5	67.6 - 135.6
Sodium, mg	4	0.54	.34 - 1.12

Nutrients determined with standard procedures: moisture, lipids, ash: using A.O.A.C., 13th ed., 1980; total kjeldahl N (TKN) method of Fukumoto and Chang, *J.A.O.A.C.* 65(5): 1076-1079, 1982 with protein = TKN x 6.25; carbohydrate by difference, B-carotene courtesy of Dr. J.N. Thompson, Health Protection Branch, Ottawa, using HPLC techniques; folacin courtesy of Dr. K. Hoppner, Health Protection Branch, Ottawa, using microbiological method; minerals by ICPAES on dry matter, ascorbate by the method of Pelletier and Brassard reported in *J. Food Sci. 42:1471-1474, 1977.*

PHOTOTOXIC COMPOUNDS IN *HERACLEUM LANATUM*

There are numerous reports of dermatitis induced by skin contact with leaves, shoots, fruits and roots of various species of *Heracleum*, especially when this contact occurs in sunlight (Mitchell and Rook, 1979). The overview of Mitchell and Rook (1979) demonstrates the wide body of literature that exists on the irritant properties of the 70 species of *Heracleum* found in north temperature regions and on tropical mountains. The chemistry of the toxin has also been studied extensively, resulting in the identification of four furanocoumarins: psoralen, xanthotoxin (8-methoxypsoralen), bergapten (5-methoxypsoralen) and angelicin. Skin damage is caused by complexing of the furanocoumarins with thymine residues of DNA, thereby interfering with normal DNA repair and inducing mutations. Extracts of *Heracleum lanatum* were shown to inhibit DNA repair in human fibroblasts irradiated with ultraviolet light (Stich *et al.* 1975; Scheel 1973).

Because none of the literature reviewed to date takes into account the Indian process of peeling the stalks before using them for food, we explored the possibility that peeling would reduce the furanocoumarin content of this vegetable. Samples of *Heracleum lanatum* were harvested and peeled as previously described. Samples of unpeeled and peeled stalks were analyzed, as were the stalk peelings. Ten-gram (fresh weight) samples were macerated and extracted overnight with hot 80% methanol. After adjusting to 60% methanol, a rotary evaporator was used to bring the sample to an aqueous residue. This was extracted overnight with an equal volume of ether. The furanocoumarin-rich ether layer was separated and evaporated, and the residue dissolved in a small amount of methanol. These were spotted on TLC silica gel sheets and developed in 100% chloroform and then hexane:pentane:ethyl acetate (35:35:30). The dried chromatograms were inspected in 360 nm light and the spots marked, scraped, and extracted into methanol. Absorbance was measured in a Beckman DB-G spectrophotometer and quantities were estimated with extinction curves prepared with standards.

As seen in Table 4, *Heracleum lanatum* at both sites of harvest contained furanocoumarins. Peeled samples from site 1 contained less than half the total furano-

Table 4.—Furanocoumarins* in peeled and unpeeled stalks of *Heracleum lanatum*.

Areas sampled/ Preparation	Xanthotoxin	Bergapten	Angelicin	Total
		mg/100 g fresh weight		
Site 1				
Unpeeled	3.8	2.2	3.6	9.6
Peelings	4.6	2.6	3.2	10.4
Peeled	2.0	.8	1.6	4.4
Site 2				
Unpeeled	18.8	9.6	26.8	35.2
Peeled	8.0	2.0	6.8	16.8

*A fourth furanocoumarin, psoralen, was not analyzed.

coumarins of the peelings. At site 2, peelings were not available, but the peeled stalks again contained close to half the total furanocoumarin content of the unpeeled stalks. In the stalks from site 1, the major agent was xanthotoxin, whereas from site 2 the major agent was angelicin.

Ivie *et al.* (1981) note that as little as 5 mg of certain psoralens taken internally in one dose of medication could cause physiological effects related to skin depigmentation. Although the quantitative effects of the compound reported here are not known, nor are their cumulative effects, it is possible that 4-16 mg of total furanocoumarins eaten in 100 g of peeled vegetable product may cause adverse effects. However it is salient to consider that 100 g of cow-parsnip represents consumption of approximately 10 peeled stalks. From our discussions with native people, only 1 or 2 stalks would be eaten at any one time, although during the season when they are ready for harvest this amount may be eaten as often as 3 or 4 times per week.

Native people were, and still are, quite aware of the harmful effects of eating peelings of cow-parsnip, or of eating too much of it, especially during pregnancy or infancy. It would appear that peeling the stalks before consumption reduced or minimized these effects.

SUMMARY

Cow-parsnip *(Heracleum lanatum)* is a fascinating vegetable used by native people of northwestern North America. It was widely used as a food by many native people, who were aware of its potential toxicity. Nutrient levels of the peeled stalks are similar to those of marketed foods in kind, particularly celery, as are the taste and texture qualities. Women of the Nuxalk Nation of Bella Coola, B.C. reported a declining use of this wild plant food during this century, and a lower taste appreciation score on the part of younger women in comparison to older women.

Analysis of furanocoumarins in the peeled stalks, in comparison to unpeeled stalks and the peelings, show that the peeled stalks contain less than half the original furanocoumarin content. Although skin contact with the leaves or peelings, or eating large quantities of the peeled stalks, could be suspected of producing dermatitis in the presence of sunlight, or other unknown effects, native people apparently moderated their exposure so that the nutritional, sensory and cultural benefits of use of this plant could be enjoyed.

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RECENT DOCTORAL DISSERTATIONS OF INTEREST TO ETHNOBIOLOGISTS: FALL 1985 - FALL 1986

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Dates covered by the present paper include: Volume A (Humanities and Social Sciences): September 1985-October 1986; Volume B (Sciences and Engineering): October 1985-October 1986; Volume C (European Dissertations): Winter 1986-Summer 1986. Note that these are the dates for the issues of D.A. in which the abstracts appear, rather than the dates of acceptance of the dissertations themselves. A few older dissertations overlooked in previous columns have also been included.

The dissertations are listed alphabetically below, along with the year of acceptance, title, institution, length, advisor or major professor, number of the page in D.A. in which the abstract may be found, University Microfilms order number, and the ISBN number, when this information was given.

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NEWS and COMMENTS

POETRY CORNER

Ethnobiology inspires poetic license: Anthropologists and other scholars have spilled gallons of ink to cover a topic that David Fraser's verse *(BioScience,* May 1985) captures in 20 lines.

TAXONOMY IN DEUTERONOMY
by David Fraser

When God declared to Moses that the pig was out of season,
It was not for trichinosis or some philanthropic reason.
He'd simply had a second look at some of His creations,
And found among them several taxonomic abetrations.

An ungulate must have a cloven hoof, the Lord pronounced,
And chew its dinner twice before the next meal is announced,
But should it have a simple hoof, or fail to chew its cud,
It falls outside the narrow lines of ungulatic blood.

In likewise manner, said the Lord, the creatures of the air
Should look on insects, fruit and seeds as their appointed fare,
But those that feed on fish or flesh, they fall outside the system:
All falcons, ospreys, crows and owls (and He went on to list them).

The creatures of the sea, He said, and neighbors thereabout
Should all be clad in scales and fins to keep the dampness out,
But crab and lobster, squid and snails, though they may look delicious,
And others without fins and scales—are foul, and not fishes.

Now, Cain and Abel founded phenotypic variation,
And Noah showed the wisdom of genetic preservation,
But it was God himself who taught that taxonomic purity
Depends on arbitrary groups and pentateuchal surety.

David Fraser is an ethologist in Agriculture Canada's Animal Research Centre, Ottawa.

While we are on the subject of foods, tabooed and otherwise, savor this literary hors d'oeuvre:

The parsnip, children, I repeat,
  is simply an anemic beet.
Some folks claim the parsnip's edible;
  I myself find this claim incredible.

(I thank Ed Bakewell, a student in my Plants, Animals, and People class at the University of Washington, for calling this Ogden Nash gem to my attention.)

NEWS ITEMS

Chernobyl rain fatal to Lapp's reindeer harvest. Two days after the 26 April 1986 nuclear disaster at Chernobyl heavy spring rains deposited a dangerous dose of radioactive iodine and cesium on the Baltic coastline north of Stockholm, raising local radiation levels to 80 times normal. As documented in the National Geographic's Explorer series presentation "The Day the Reindeer Died" (WTBS, Atlanta), up to 30,000 contaminated reindeer had to be slaughtered, then discarded, casting a pall over the Lapp's traditional annual reindeer harvest festivities. Other traditional Lapp foods such as wild berries and mushrooms were also too "hot" to be eaten. A reminder of our precarious global interdependence. *(Seattle Post-Intelligencer,* 17 October 1986).

Goldenseal herb in demand amoung drug users. Goldenseal *(Hydrastis canadensis,* Compositae), a wild herb native to eastern North America, is reported to be a fast-moving item at natural herb outlets in west coast ports. Navy crews are now subject to routine urinalysis to detect illicit drug use, and many sailors believe that goldenseal will help them pass their drug tests. Goldenseal is described in *Dr. Kurt Donsbach's Herbs, Revised* as "acting as a blood purifier so well that certain drugs are completely removed from the body within hours. . ." However, SYVA Co. of Palo Alto, Calif., which manufactures lab kits for drug testing, has evaluated these claims and discounts them, despite goldenseal's considerable diuretic potency. *(Seattle Post-Intelligencer,* 23 October 1986).

ANNOUNCEMENTS

TENTH ANNUAL ETHNOBIOLOGY CONFERENCE GAINESVILLE, FLORIDA

The Tenth Annual Ethnobiology Conference will be held 5-8 March 1987 at the Florida State Museum in Gainesville. An ethnic banquet and field trips to St. Augustine and the St. Johns River are planned. For the first time a special prize will be presented for the best student paper submitted for the conference. The award is given in honor of Barbara Lawrence, late curator of Mammals at the Museum of Comparative Zoology. To pre-register send a check (regular $35, student $25) by 10 February to: 10th Ethnobiology Conference, DOCE-UF, 1938 W. University Ave., Gainesville, FL 32603.

LAWRENCE AWARD OFFERED

The Society of Ethnobiology will offer an award in honor of Barbara Lawrence for the best paper submitted by a student for presentation at the annual meeting. Papers in any subject within the realm of ethnobiology are eligible for the competition. The prize will be a significant honor for the recipient and comes with a substantial monetary award.

The prize honors Barbara Lawrence who was curator of Mammals at the Museum of Comparative Zoology. She was one of the first to recognize the potential of collections of animal remains associated with human habitation sites and was on the forefront of advocating analysis of these assemblages. Her work on aboriginal dogs and her studies of faunal remains excavated from sites in Turkey are particularly well known.

The high standards held by Barbara Lawrence will be the guiding principle in choosing the award winning paper. The competition is open to any member who is a

student (held the PhD degree less than one year). Papers submitted for this competition can be presented in an oral or a poster session.

Manuscripts submitted for this competition must be single authored; joint efforts will not be considered. Manuscripts are judged solely on quality, originality, and presentation of research. They should follow the Journal of Ethnobiology format and should be sufficiently precise and documented to enable the reviewing committee to judge their merits. Manuscripts are limited to eight double-spaced, typed pages, including a required abstract but excluding copies of figures, tables and references.

Papers submitted for this competition should be accompanied by a cover letter indicating that you are a Society member and meet the criteria listed above. The deadline for receipt of your paper is the 5th of January 1987. Please send it to the 10th Ethnobiology Conference, DOCE-UF, 1938 W. University Ave., Gainesville, FL 32603.

The Society must raise $5000 to endow this prize. Contributions are tax deductible. Address your checks to the Society of Ethnobiology: Lawrence Fund and mail them to Elizabeth S. Wing, Curator of Zooarchaeology, Florida State Museum, Gainesville, FL 32611.

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This non-profit institution maintains an extensive library of books, reports, photos, and monographs on cacti and succulents. They operate a botanical garden near Bisbee, Arizona, where shade frames and greenhouses foster thousands of plants of 650 varieties, including threatened and endangered species. One goal of the organization is to restock these fragile wild populations. Volunteers are available to provide free tours and presentations at the garden. A slide presentation and lecture entitled "Indian Use of Desert Plants" is available. For more information contact Arizona Cactus and Succulent Research, Inc., R. R. 1, Box 8, Mulberry Lane, Bisbee, Arizona 85603, or call (602) 432-7040.

SCHOOL OF AMERICAN RESEARCH

The School of American Research has granted a one-year research residency fellowship to Thomas Killion, Ph.D. candidate at the University of New Mexico, for the preparation of a monograph on the practice of horticulture by tropical agriculturalists. For more information on this project or details on the School of American Research Weatherhead Foundation fellowships, write P.O. Box 2188, 660 Garcia Street, Santa Fe, New Mexico 87501, or call (505) 982-3583.

THE JACOBS RESEARCH FUNDS
WHATCOM MUSEUM SOCIETY
121 Prospect St., Bellingham, WA 98225

SCOPE OF RESEARCH SUPPORT: Grants are awarded for research in the field of social and cultural anthropology among living American native peoples. The primary focus is on the Pacific Northwest, but other regions of North America will be considered. Field studies which address cultural expressive systems, such as music, language, dance, mythology, world view, plastic and graphic arts, intellectual life, and religion, including ones which propose comparative psychological analysis, are appropriate. (Projects in archaeology, physical anthropology, applied anthropology, and applied linguistics are not eligible, nor is archival research supported.)

SIZE AND DURATION OF GRANTS: Grants will not ordinarily exceed $1200. They are made to individuals for a maximum period of one year and can be renewed only on the basis of a new application. Applications due by February 15th.

ETHNOZOOLOGY SPECIALIST GROUP FORMED

The Ethnozoology Specialist Group of the Survival Species Commission of the International Union for the Conservation of Nature (IUCN) was established early in 1986 and issued its first newsletter last March. Bertrand des Clers will chair the Ethnozoology Specialist Group. In his capacity as director of the International Foundation for the Conservation of Game (IGF) he attended meetings last year sponsored by the FAO African Forestry and Wildlife Commission in Bamako, Mali, of the International Council for Game and Wildlife Conservation (CIC) in Manaus, Brazil, and of the World Conservation Strategy Conference in Ottawa, Canada. The IUCN is preparing a program on traditional knowledge under the direction of Mr. Jeffrey McNeely of the Program and Policy Division of IUCN. For more information or to join this group reply to Bertrand des Clers, Chairman, ETZ/SSC, 16/18 Avenue Rachel, 75018, Paris, France.

NINTH ANNUAL MEETING SOCIETY OF ETHNOBIOLOGY

20-23 MARCH 1986
University of New Mexico
Albuquerque, New Mexico

LOCAL COMMITTEE:
Mollie Toll and Karen Clary (Co-chairpersons);
K. Carnean; M. Caffey; B. Crowder; A. Cully; P. Knight.

ABRIDGED MINUTES of MEETING of EDITORIAL BOARD

This year's meeting was attended by President Steve Weber, Secretary/Treasurer Steve Emslie, Editor Willard Van Asdall, Associate Editor Karen Adams, Eugene Hunn, Charles Miksicek, Brent Berlin, Robert A. Bye, Jr., Richard Ford, Harriet Kuhnlein, Jan Timbrook, Amadeo Rea, Elizabeth Wing, Mollie Toll, and Karen Clary. The meeting took place on 22 March between 1930 and 2200 h.

The meeting was called to order at 1930 by Pres. Steve Weber. Steve Emslie began with the Treasurer's report for the period 7 April 1985-20 March 1986. The Society membership consists of 198 individual members and 96 institutions. Fund raising at the ninth conference added an additional 19 members to the Society, and $453.50 from sales of Society T-shirts. The printing of these shirts cost $109.75. The outstanding 1986 conference was attended by 148 paid and 24 gratis registrants for an official attendance of 172.

Richard Ford raised the question of how to increase the institutional membership of the Society. Board members agreed that individuals must be persuaded to request their institutional libraries to subscribe to the *Journal of Ethnobiology* if they do not do so already. Willard Van Asdall agreed to design a circular to mail to individuals and institutions that will help promote the Society. Board members agreed to cooperate in obtaining mailing lists from other Societies and by sending these lists to Willard. Robert Bye will contact the editor of *Current Contents* to have the journal listed in this publication.

The next subject of discussion concerned the journal. Willard stated that more papers need to be submitted to create a backlog. This will avoid future delays in publication. Vol. 5 No. 2 is now in press and will be released shortly. This issue will include the journal's first complete index of all previous issues. Reprints of this index will also be available and will be sold at $2.00 each.

Vol. 6 No. 1 will be the Society's first special issue consisting of a series of solicited papers on "New Directions in Ethnobiology". At least nine papers will be in this issue. Five other papers are now under review for Vol. 6 No. 2. Charles Miksicek reports several book reviews are completed for the next issue, but is resigning as book review editor and a replacement is needed.

Eugene Hunn reports that more information is needed for the News and Comments section of the journal. Suggestions for expanding this section included the addition of notices of work in progress by ethnobiologists, and a listing of recently published papers related to ethnobiology.

Amadeo Rea suggested that a 'Notes' section be added to the journal for brief articles. The board agreed that this is needed and that these notes will also be reviewed similar to other articles submitted to the journal. This section will be initiated in the next issue possible; board members were invited to submit brief articles for review.

The next area of discussion concerned the conferences. Mollie Toll and Karen Clary will communicate suggestions and advisement to Elizabeth Wing, chairperson for the

tenth conference to be held in Gainesville, Florida, 5-8 March 1987. Robert Bye will host the eleventh conference in Mexico City. The twelfth conference is tentatively planned for Riverside, California, and the thirteenth for Washington, D.C. (Smithsonian Institution). Jan Timbrook will communicate with M. Toll regarding suggested changes to the conference handbook.

Society organization was discussed last. Brent Berlin suggested the implementation of the Society by-laws. A complete revision of the by-laws will be produced jointly by Steve Weber and Brent Berlin and sent to all board members for approval. Richard Ford suggested a re-organization of the Society into two bodies: (1) a Society Board of Directors composed of a President, Secretary/Treasurer, and three Board members to be elected by the Society membership, and (2) an Editorial Board consisting of an Editor appointed by the Board of Directors, and an editorial board appointed by the Editor and approved by the Board of Directors. A nominating committee was formed to determine whose names will be placed on the ballot for the Board of Directors. Steve Emslie, Steve Weber, and Charles Miksicek agreed to serve on this committee which will meet once to determine the ballot. All Society members will be mailed a ballot in the fall, 1986, with the results of the election to be announced at the tenth conference. The newly elected Board of Directors will begin their 2-year term at the tenth conference. Finally, Robert Bye motioned to extend a vote of confidence and appreciation to Willard Van Asdall for his excellent job with the journal. The board unanimously approved the motion.

The only new business to be discussed concerned the offer by Westview Press to publish a monograph series for the Society. The board accepted this offer and Richard Ford and Paul Minnis agreed to prepare a detailed proposal with Westview Press to be circulated to the board.

The meeting was adjourned by Pres. Steve Weber at 2200 h.

SOCIETY OF ETHNOBIOLOGY
TREASURER'S REPORT 20 MARCH 1986

DATE OF LAST REPORT: 7 April 1985
FUNDS IN ACCOUNT AT LAST REPORT: $634.97

INCOME SINCE LAST REPORT:

Sale of Backissues:	
Vol. 1	$323.75
Vol. 2	352.00
Vol. 3	309.00
Vol. 4	337.50
Subscriptions:	
Vol. 5	4,931.44
Vol. 6	3,035.00
Author's Reprints:	
Vol. 4	170.00
Vol. 5	550.00
Sale of T-shirts	126.00
1985 Conference Registration	552.50
Interest	161.51

TOTAL INCOME THIS REPORT: $10,848.70

TOTAL INCOME + PREVIOUS FUNDS IN ACCOUNT: $11,483.67

EXPENSES SINCE LAST REPORT:

	Vol. 5 No. 1	Vol. 5 No. 2
Typesetting	$1131.00	$1300.00
Printing	$2639.75	$1800.00 (not complete)
Postage	243.59	150.00 (not complete)
TOTAL	$4014.34	$3250.00

OTHER EXPENSES:

Postage and Petty Cash	197.00
Corporation Fee	10.00
Advertising	43.18
Overpayment refunds	293.00

TOTAL EXPENSES SINCE LAST REPORT: $7807.52

TOTAL INCOME IN ACCOUNT—TOTAL EXPENSES: $11,483.67-7807.52 = $3676.15

TOTAL FUNDS IN ACCOUNT AS OF 20 MARCH 1986: $3694.46

CIRCULATION	Last Report	This Report
INDIVIDUALS	206	198
INSTITUTIONS	91	96

BY-LAWS OF THE SOCIETY OF ETHNOBIOLOGY

ARTICLE I, Name.

The name of this organization shall be the Society of Ethnobiology, hereinafter referred to as the Society.

ARTICLE II, Objectives.

The objectives of this organization shall be to establish and maintain an organization of scientists of high standing with a common interest in ethnobiology, to promote the discussion and communication of knowledge devoted to the interdisciplinary study of anthropology and biology, and to stimulate and disseminate research advances concerning ethnobiology by sponsoring scientific and professional publications.

ARTICLE III, Office.

The registered office of the society shall be located in the STATE OF ARIZONA at such place as may be fixed from time to time by the Board of Directors upon filing of such notices as may be required by law, and the registered agent shall have a business office identical with such registered office.

ARTICLE IV, Membership.

The membership of the Society shall consist of any person, firm, unincorporated association or corporation that has paid its membership fee.

ARTICLE V, Dues.

All members will be required to pay annual dues. Annual dues shall be set by the Board of Directors.

ARTICLE VI, Rights and Prerogatives of Members.

The rights and prerogatives of members are those customary to the members of any organization, including participation in meetings, discussion, committee membership, and the right to vote or hold elective office.

ARTICLE VII, Meeting of Members.

1) An annual meeting of the members shall be held at a time and place as may be designated by the Board of Directors.

2) The annual conference is a scientific program, arranged to reflect the broad interests of Society members in the field of ethnobiology. The annual meeting should provide opportunities for members and others attending to hear reports of recent research in ethnobiology, exchange material of mutual interest, and to discuss matters of policy relating to the Society. Issues relating to business or policy should be freely discussed

among all members present in one session of the Society's annual conference at an annual business meeting. Persons considering membership in the Society shall be permitted to attend and participants in the program shall be encouraged to join the Society.

3) A Board Meeting shall be held at each annual conference.

ARTICLE VIII, Organizational Structure and Procedures.

1) The affairs of the Society shall be managed by its Board of Directors. Directors need not be residents of the STATE OF ARIZONA but must be members of the Society.

2) The Directors with voting power shall consist of 6 elected officers, a President, a Vice-President/President-Elect, a Secretary/Treasurer, and 3 Board Members. There shall be one appointed officer, the editor of the Journal of Ethnobiology, who shall be appointed by the President in consultation with the Board. The elected officers of the Society, and the editor of the Journal, and past Presidents, shall constitute the Board of Directors. Past Presidents' and editors' participation is in an advisory capacity only and they will have no voting power. Past Presidents shall retain the right to be heard and to participate at all Board and Committee meetings, whether open to the public or not.

3) Terms. The editor of the Journal of Ethnobiology serves an unspecified term on a voluntary basis. The Secretary/Treasurer serves two consecutive years and may be re-elected. The Vice-President/President-Elect shall be elected for a two-year term as Vice-President and then succeed to the office of President for two more years and may not be re-elected. The 3 Directors-at-large shall be elected for a 3-year term and may be re-elected for no more than 2 terms. Directors-at-large shall be elected each year with the purpose of ensuring that no one 3-year term overlaps with another. The organizational year is from annual meeting to annual meeting. If, for any reason, an annual conference does take place, the organizational year shall run temporarily from January through December.

4) Elections. Annual elections are held by written ballot. All individual members in good standing may vote in the election.

5) Nominations. The President appoints and serves as Chair of a nominating committee consisting of a total of 3 persons. This committee shall solicit members in good standing willing and able to serve as officers and Board Members for the forthcoming year (beginning at the following conference). The resulting report will appear as a ballot no later than 3 months before the end of the current year and will be sent to all members.

6) Officers. Duties of all officers will be those customarily associated with their titles or as listed in Robert's Rules of Order, or by action of the Board of Directors. The President shall be responsible for coordinating organizational activities, including an annual meeting, and for making decisions regarding other organizational business in consultation with other members of the Board of Directors. A consensus of the Board as a group will decide final recommendations in all such instances. The Secretary/Treasurer shall be responsible for receiving dues, managing the Society's funds, and disbursing same. This person shall be authorized to sign checks for the organization. The Secretary/Treasurer shall further be responsible for reporting to the

Membership and/or Board of Directors current and projected financial situations of the Society. The Vice-President/President-Elect shall be responsible for assisting the President and for taking a major role in the organization of the annual meeting. The editor of the Journal of Ethnobiology shall oversee the running of the Journal as well as the appointment of the members of its editorial board. The editor may act independently of the Society Board of Directors in matters concerning the Journal.

ARTICLE IX, Finance.

1) The monies of the Society shall be deposited in the name of the corporation in such bank or banks, or trust company or trust companies, as the Board of Directors shall designate, and shall be drawn out only by checks signed by person or persons designated by resolution of the Board of Directors.

2) The Society shall continue to act in good faith towards debts and obligations incurred by the Society to past and present officers of the Board of Directors.

ARTICLE X, Books and Records.

The Society shall keep correct and complete books and records of accounts and shall keep minutes of the proceedings of Board meetings of the Directors and committees having any of the authority of Board of Directors' Meetings.

ARTICLE XI, Amendments.

Upon recommendation by the Board of Directors or by not less than one third of the members, these by-laws may be altered, amended, or repealed and new by-laws may be adopted by a majority vote at a regular annual meeting.

NOTICE TO AUTHORS

The *Journal of Ethnobiology* accepts papers on original research in ethnotaxonomy and folk classification, ethnobotany, ethnozoology, cultural ecology, plant domestication, zooarchaeology, archaeobotany, palynology, dendrochronology and ethnomedicine. Authors should follow the format for article organization and bibliographies from articles in this issue. All papers should be typed double-spaced with pica or elite type on 8½ x 11 inch paper with at least one inch margins on all sides. The ratio of tables and figures to text pages should not exceed 1:2-3. Tables should not duplicate material in either the text or graphs. All illustrations are considered figures and should be submitted reduced to a size which can be published within a journal page without further reduction. Photographs should be glossy prints of good contrast and sharpness with metric scales included when appropriate. All illustrations should have the author(s) name(s) written on the back with the figure number and a designation for the top of the figure. Legends for figures should be typed on a separate page at the end of the manuscript. Do not place footnotes at the bottom of the text pages; list these in order on a separate sheet at the end of the manuscript. Metric units should be used in all measurements. Type author(s) name(s) at the top left corner of each manuscript page; designate by handwritten notes in the left margin of manuscript pages where tables and graphs should appear.

If native language terminology is used as data, a consistent phonemic orthography should be employed, unless a practical alphabet or a more narrow phonetic transcription is justified. A brief characterization of this orthography and of the phonemix inventory of the language(s) described should be given in an initial note. To increase readability native terms should be indicated as ***bold-face italics*** to contrast with the normal use of *italic* type for foreign terms, such as latin binomials. If necessary, the distinction between lexical *glosses*, i.e., English language approximations of a term's referential meaning, and precise English equivalents or definitions should be indicated by enclosing the gloss in single quotation marks.

Authors must submit two copies of their manuscript plus the original copy and original figures. Papers not submitted in the correct format will be returned to the author. Submit your manuscripts to:

DR. WILLARD VAN ASDALL, Editor
Journal of Ethnobiology
Arizona State Museum, Building 26
University of Arizona
Tucson, Arizona 85721

NEWS AND COMMENTS

Individuals with information for the "News and Comments" section of the *Journal* should submit all appropriate material to Eugene Hunn, Department of Anthropology, DH-05, University of Washington, Seattle, Washington 98195.

BOOK REVIEWS

We welcome suggestions on books to review or actual reviews from the readership of the *Journal*. Please send suggestions, comments, or reviews to Terence E. Hays, Department of Anthropology and Geography, Rhode Island College, Providence, Rhode Island 02908.

SUBSCRIPTIONS

Subscriptions to the *Journal of Ethnobiology* should be addressed to Cecil Brown, Department of Anthropology, Northern Illinois University, Dekalb, Illinois 60115. Subscription rates are $35.00, institutional; $17.00 regular members, for U.S., Canada, and Mexico; foreign subscribers add $8.00. Write checks payable to *Journal of Ethnobiology*. Defective copies or copies lost in shipment will be replaced if written request is received within one year of issue.

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Journal of Ethnobiology

VOLUME 7, NUMBER 1 SUMMER 1987

Journal and Society Organization

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Journal of Ethnobiology is published semi-annually. Manuscripts for publication, information for the "News and Comments" and book review sections should be sent to the appropriate editor on the inside back cover of this issue.

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VOLUME 7, NUMBER 1 SUMMER 1987

SOCIETY OF ETHNOBIOLOGY

ELEVENTH ANNUAL CONFERENCE

Mexico City, Mexico

March 17-19, 1988

Please see page 119 of this issue for details.

County Fair time. By the time I was old enough to attend the County Fair, I was aware that my parents were older than those of my classmates. They did things differently, had different ideas and different values. We went to the County Fair to enjoy the atmosphere, not to compete. Blue ribbons meant nothing to my folks. My mother knew the products of her kitchen were the best and my dad was not about to be bothered with record keeping. We were always glad to have the County Fair roll around—the crops had been laid by and some activity was needed. But there was yet another reason to anticipate the first day of County Fair. My dad, who never attended, searched the watermelon patch and always found the first of the never ending supply of perfectly ripened, perfectly sweetened, and, by the time we arrived home in late afternoon, perfectly chilled watermelons. We never tired of them and late in the fall, so as to extend the season after frost, would bury dozens of them under hay.

My parents always saved their own seed. This in itself was almost a ritual. There was much discussion as to whether a particular melon was the best of the season and, if it was, a notation was placed on previously saved packets of seed indicating that they had been demoted. In February all seed packets were brought out and examined. Mom and Dad visited the seed store in town and then there would be more discussion as to whether to use their own seed or purchase new. My mother, ever anxious about what today would be called "quality control" wondered if perhaps some of the seed might be old and of low germinability. So in the end they planted the seeds from the best fruit of the previous year.

Until one year when, after much thought, they decided that although they were well satisfied with the "Tom Watson" and "Kleckley Sweet" varieties they had raised for years, they might buy a small packet of a new watermelon in the seed store. It was called "Irish Grey." And it displaced the old standards in family. The next year a much larger part of the patch went into Irish Grey watermelons and then the selection process began anew—saving the best of the best of the best.

Neighbors and passers-by speculated as to why my folks had the best watermelons around. They knew about the cold frame and the early start and individual care the plants thereby received. They could easily observe that the watermelon patch was located on the least fertile and sandiest soil on the farm. They knew—for they could see from afar—that "stuff" (soil amendments, it would be called today) was added to each hill. But one thing they didn't know was the talent my folks had for recalling the taste watermelons earlier in the season had had and comparing their flavors and textures with that of the one currently being enjoyed. Therein lay a good part of their "secret."

Mom and Dad never bothered to save seeds of the earliest maturing watermelon unless it was also the best tasting. They depended upon subtle alterations of the micro-environment, including the cold frame, to promote early and rapid growth so that we could have the first watermelons in the township and continue with the tradition of enjoying a home grown treat following an afternoon at the County Fair.

—W.V.

J. Ethnobiol. 7(1):1-12 Summer 1987

ESTIMATION OF BODY WEIGHT OF WHITE-TAILED DEER *(ODOCOILEUS VIRGINIANUS)* FROM BONE SIZE

JAMES R. PURDUE
Zoology Section
Illinois State Museum
Corner Spring and Edwards Streets
Springfield, Illinois 62706

ABSTRACT.—Lower leg bones of 210 modern white-tailed deer *(Odocoileus virginianus)* from 8 localities in eastern North America were used to examine factors influencing the relationship between bone size and body size. Locality (island versus mainland), age, and sex were shown to affect bone size-body weight relationships. After accounting for the compounding factors, a set of unified regressions are presented that estimate adult live weight in the late autumn of white-tailed deer. However, since live weight is subjected to many diverse influences, the estimates should be used only as an ordinal guide to size.

INTRODUCTION

White (1953) pioneered the use of the minimum number of individuals (MNI) and meat weight to characterize the animal portion of human subsistence. In White's system, the MNI of each prey species was multiplied by its "average" meat weight (a percentage of average body weight), producing an index that indicated the relative contribution of each species to human diet. Although both MNI and meat weight became widely used in archaeozoological analyses, their shortcomings also became apparent. Problems associated with, and suggested alterations to, MNI have been addressed in a number of studies (Casteel 1977; Grayson 1973, 1978, 1979; Martin 1983; for reviews, see Grayson 1984; Klein and Cruz-Uribe 1984). Fieller and Turner (1982) proposed a probabilistic alternative to MNI (but see, Klein and Cruz-Uribe 1984).

Few studies have critically evaluated "average" meat weights. However, Smith (1975) noted the effects that sex, age, and location have on weight in white-tailed deer *(Odocoileus virginianus).* After (1) showing that deer from a study site had a balanced sex ratio and (2) determining the age profile from tooth eruption and wear of the mandibles, Smith (1975) used sex-pooled, age-specific mean dressed weights of the modern local deer to produce meat weight estimates. Emerson (1978, 1983), taking another approach, estimated dressed weights of deer from archaeological contexts using a regression that related the length of the astragalus to dressed weight in a sex- and age-pooled sample of modern white-tailed deer.

Recent reviews of the quantification techniques commonly used in archaeozoological analyses have noted the numerous biases and deficiences of methods, in general, and of meat weight, in particular (Grayson 1984; Klein and Cruz-Uribe 1984; Lyman 1979). Lyman (1979) notes confusion in the terminology of various weight measures (e.g., live weight, available meat, consumed meat) and advocates the use of butchering units. Klein and Cruz-Uribe (1984) dismiss meat weight as adding no significant new information about the relative importance of various taxa at a site, while Grayson (1984) takes the view that meat weight based on MNI (as opposed to bone weight) may have some validity as an ordinal measure of faunal usage.

Even though the use of estimated meat weight as traditionally applied may have limited utility, certain other research problems can still benefit by reliable projections of live or body weight (leaving to others the decision of what proportion of weight to allocate to available and to consumed meat). For example, biomass, or its conversion to calories, can be used to evaluate the benefits of many theoretical cost-benefit models of human subsistence (Earle and Christenson 1980). Since some animal species are known to show shifts in body size through time in the Holocene (Purdue 1980, 1986), then those shifts may require consideration in such models. Measures (e.g., bone width, factor scores based on morphological measurements) other than live weight, or calories derived from body weight, cannot easily be compared between taxa. Future studies that contrast the relative contributions of plant and animal resources to human diet (currently an important, but neglected issue) may find derived calories from meat weight an important measure, in spite of its many faults.

The objectives of this study are to thoroughly examine the relationship between bone measurement and live weight in white-tailed deer, an important terrestrial prey item of prehistoric humans in eastern North America, and, after accounting for as many sources of variation as possible, to present equations that accurately estimate live weight.

METHODS

The lower legs of 207 deer from 8 localities were collected when the animals were brought to hunter check stations. Three additional specimens were killed by automobiles in central Illinois (Table 1). All animals were taken in the late fall or early winter. Live or dressed weight (to the nearest 0.45 kg), sex, and age (determined by tooth eruption

TABLE 1.—*List of localities sampled for modern white-tailed deer.*

Locality	No. of Adults[a] Male	Female	Dates Sampled
Savannah River Plant, South Carolina	3	35[b]	late Nov/early Dec 1980
Ossabaw Island, Georgia	—	22	18-20 Dec 1980
Hickory County, Missouri	2	7	18-19 Nov 1977
Macon County, Missouri	2	6	18-19 Nov 1977
Ozark County, Missouri	1	11	18-19 Nov 1977
Pope County, Illinois	1	13	18-19 Nov 1977
Illinois (central area)	0	3	Nov 1976; Feb 1977
Arkansas (statewide)	5	25	7-9; 27-28 Nov 1981

[a]males ≥3.5 yr.; females ≥2.5 yr.

[b]an additional 74 non-adults were used for some tests

and wear) were noted for each specimen. Dressed weights, which were taken for the majority of specimens from the Missouri localities, Pope County, Illinois, and Arkansas, were converted to live weights using sex- and age-specific regressions (Roseberry and Klimstra 1975). Live weights were directly taken on specimens from South Carolina and Georgia. An additional sample of 24 deer was taken from hunters in Jo Daviess County, Illinois. No weights were available for these animals, but they were useful for testing the consistency of the weight-estimating regressions between measurement sets.

Nine measurement sets on 7 bones were made to the nearest 0.01 mm using hand-held calipers (Fig. 1). The measurements were on ends of long bones or small, compact foot bones that are commonly recovered from archaeological sites. Gingerich *et al.* (1982)

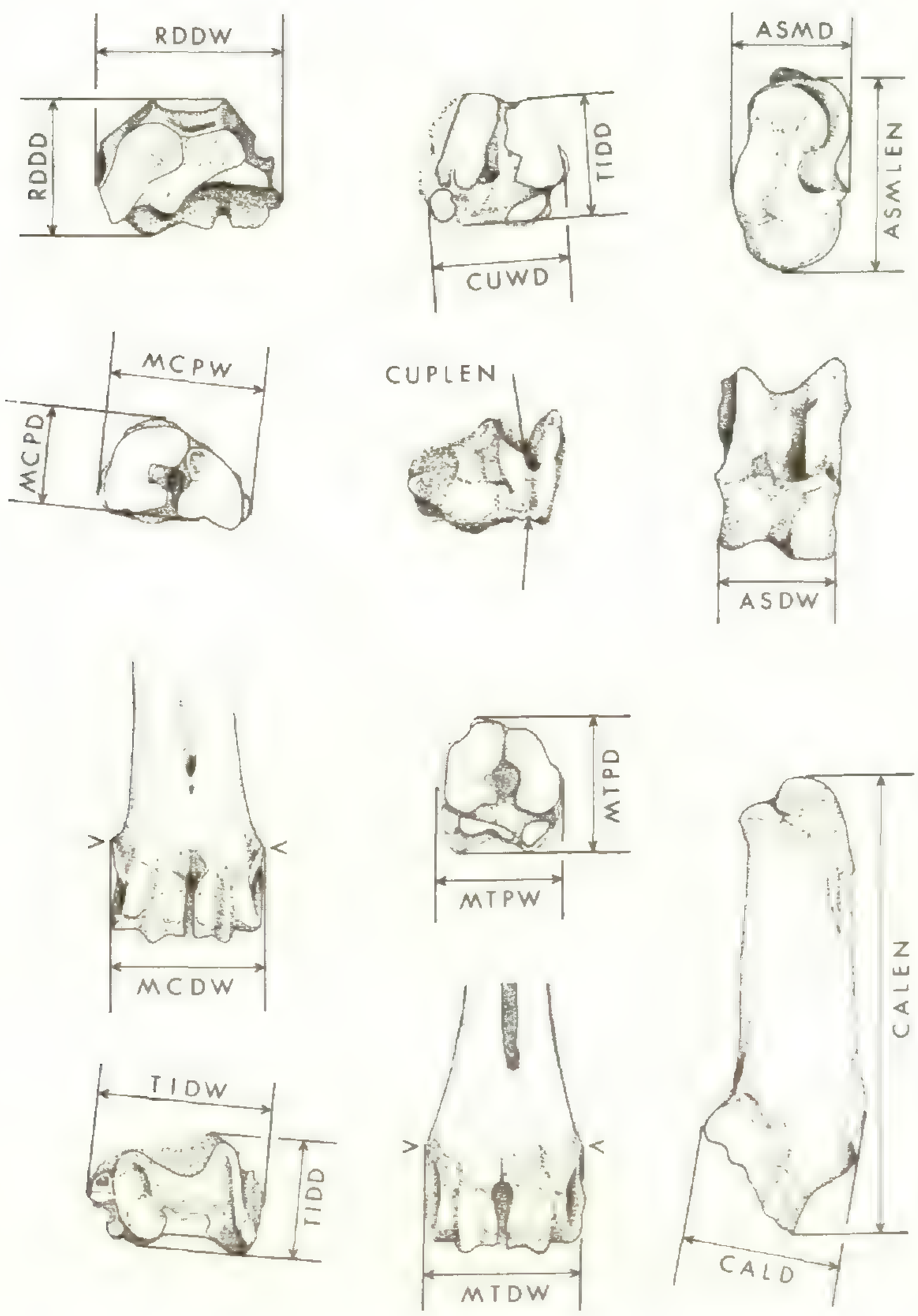

FIG. 1.—Measurements taken on leg bones of white-tailed deer. Abbreviations are: ASDW, distal width of the astragalus; ASMD, medial depth of the astragalus; ASMLEN, medial length of the astragalus; CALD, lateral depth of the calcaneum; CALEN, length of the calcaneum; CUDD, distal depth of the cuboid; CUPLEN, plantar length of the cuboid; CUWD, distal width of the cuboid; MCDW, distal width of the metacarpal; MCPD, proximal depth of the metacarpal; MCPW, proximal width of the metacarpal; MTDW, distal width of the metatarsal; MTPD, proximal depth of the metatarsal; MTPW, proximal width of the metatarsal; RDDD, distal depth of the radius; RDDW, distal width of the radius; TIDD, distal depth of the tibia; and TIDW, distal width of the tibia.

found that the crown tooth area (tooth length x width) of primates improved the estimation of body weight, relative to that calculated from simple linear measurements. Preliminary analyses of the post-cranial bones of deer yielded similar results and, consequently, cross-sectional areas or element volumes were used where possible (Table 2). For the distal ends of the metacarpal and metatarsal, preliminary analysis indicated that the depth measurements were affected by continual resculpturing of the bone long after the closing of the epiphyseal suture. Thus, it was not possible to use cross-sectional area as a measure for these elements.

Several statistical procedures were used to demonstrate the effects of biological age, sex, and locality on the relationship between bone size and body weight. Except for the age analysis, which is graphically depicted, all bone size and body weight data were

TABLE 2.—*Formulae for converting measurements into areas and volumes. Measurement abbreviations are given in Fig. 1. Abbreviations for measurement sets are: RDAR, cross-sectional area of the distal end of the radius; TIAR, cross-sectional area of the distal end of the tibia; MCPAR, cross-sectional area of the proximal end of the metacarpal; MTPAR, cross-sectional area of the proximal end of the metatarsal; ASVO, volume of the astragalus; CUVO, volume of the cuboid; and CAAR, area of the lateral surface of the calcaneum.*

Element (end)	Approximate Shape	Formula
Radius (distal)	ellipse	$RDAR = \frac{RDDW}{2} \times \frac{RDDD}{2} \times \pi$
Tibia (distal)	ellipse	$TIAR = \frac{TIDW}{2} \times \frac{TIDD}{2} \times \pi$
Metacarpal (proximal)	ellipse	$MCPAR = \frac{MCPW}{2} \times \frac{MCPD}{2} \times \pi$
Metacarpal (distal)	line	MCDW
Metacarpal (distal)	ellipse	$MTPAR = \frac{MTPW}{2} \times \frac{MTPD}{2} \times \pi$
Metatarsal (distal)	line	MTDW
Astragalus	Elliptical cylinder	$ASVO = \frac{ASMD}{2} \times \frac{ASMLEN}{2} \times ASDW \times \pi$
Cuboid	elliptical cylinder	$CUVO = \frac{CUWD}{2} \times \frac{CUDD}{2} \times CUPLEN \times \pi$
Calcaneum	ellipse	$CAAR = \frac{CALEN}{2} \times \frac{CALD}{2} \times \pi$

transformed with natural logarithms (Gingerich *et al.* 1982; Reitz and Cordier 1983). Regression analyses, conducted with the GLM Procedure of SAS (Helwig and Council 1979), were used to test the effects of sex and location on estimating weight. The REGRESSION option in SPSS (Nie *et al.* 1975) was used to produce the final regressions for predicting body weight.

RESULTS

Effect of ontogeny.—Deer of all ages from the Savannah River Plant, South Carolina were used to indicate that bones and body weight differ in growth rate (Fig. 2). The bones of the lower leg reach adult proportions much earlier in an animal's life than does body weight. In females, approximately 90% of adult bone size is reached at 6 mo. of age, whereas only 60% of the final body weight is amassed. Similar growth patterns are apparent in males, although the entire growth process is extended at least one year longer than in females. Unfortunately, the exact nature of the curves for males is not known because few males $\geq$42 mo. were available for study. Since the growth rates for bones and body weight are fundamentally different, all subsequent analyses used only animals that had achieved maximum growth, ie., females $\geq$30 mo. and males $\geq$42 mo. Males 42 mo. old have not reached their maximum weight (Severinghaus 1979), but, because of the small sample size, they were used in portions of the remaining study.

Effect of locality.—Females from Ossabaw Island, Georgia; Savannah River Plant, South Carolina; Macon and Hickory counties, Missouri; and Pope County, Illinois, were used to test if locality influenced the relationship between bone size and body weight (Table 3). Slopes are unaffected by locality. However, the Y-intercept is heterogeneous in tests of all 9 measurement sets when 5 localities are included. Significance levels for the pairwise tests of differences due to locality indicate that the Ossabaw Island sample consistently differs from two or more of the mainland samples. Brisbin and Lenarz (1984) reported other proportional differences between deer from Ossabaw Island and mainland South Carolina. They were unable to resolve which of the differential traits were phenotypic responses to environment and which were the result of natural selection.

The analyses were repeated with Ossabaw Island deer excluded (Table 3). As before, all slopes are homogeneous, but this time 6 of 9 Y-intercepts are homogeneous. Significant differences in Y-intercepts occur for the distal end of the tibia, proximal end of the metatarsal, and the cuboid. The pairwise tests indicate that the differences are between Macon County, Missouri, and the Savannah River Plant, South Carolina.

The mainland samples of deer were reasonably, but not absolutely, homogenous in the relationships between bone size and body weight. The island deer are excluded from further analyses.

Effect due to sex.—Analyses of covariance were applied to test the influence of sex on bone size-body weight relationships (Table 4). Slopes are homogeneous between the sexes on all tests. In contrast, the Y-intercepts, except for that associated with the distal end of the metacarpal, are significantly different.

Bone size-body weight regressions.—The results described above indicate it prudent to estimate body weight from sex-specific regressions based on adult deer from the mainland (Table 5). It is, however, rarely possible to control all these factors with deer remains from archaeological sites. Thus, Table 5 also contains regressions with the sexes combined (females 30 mo. old were deleted to achieve a better balanced sample size between the sexes).

The use of the regressions in Table 5 for estimating body weight is demonstrated with adult female deer from Jo Daviess County, Illinois, and the Savannah River Plant,

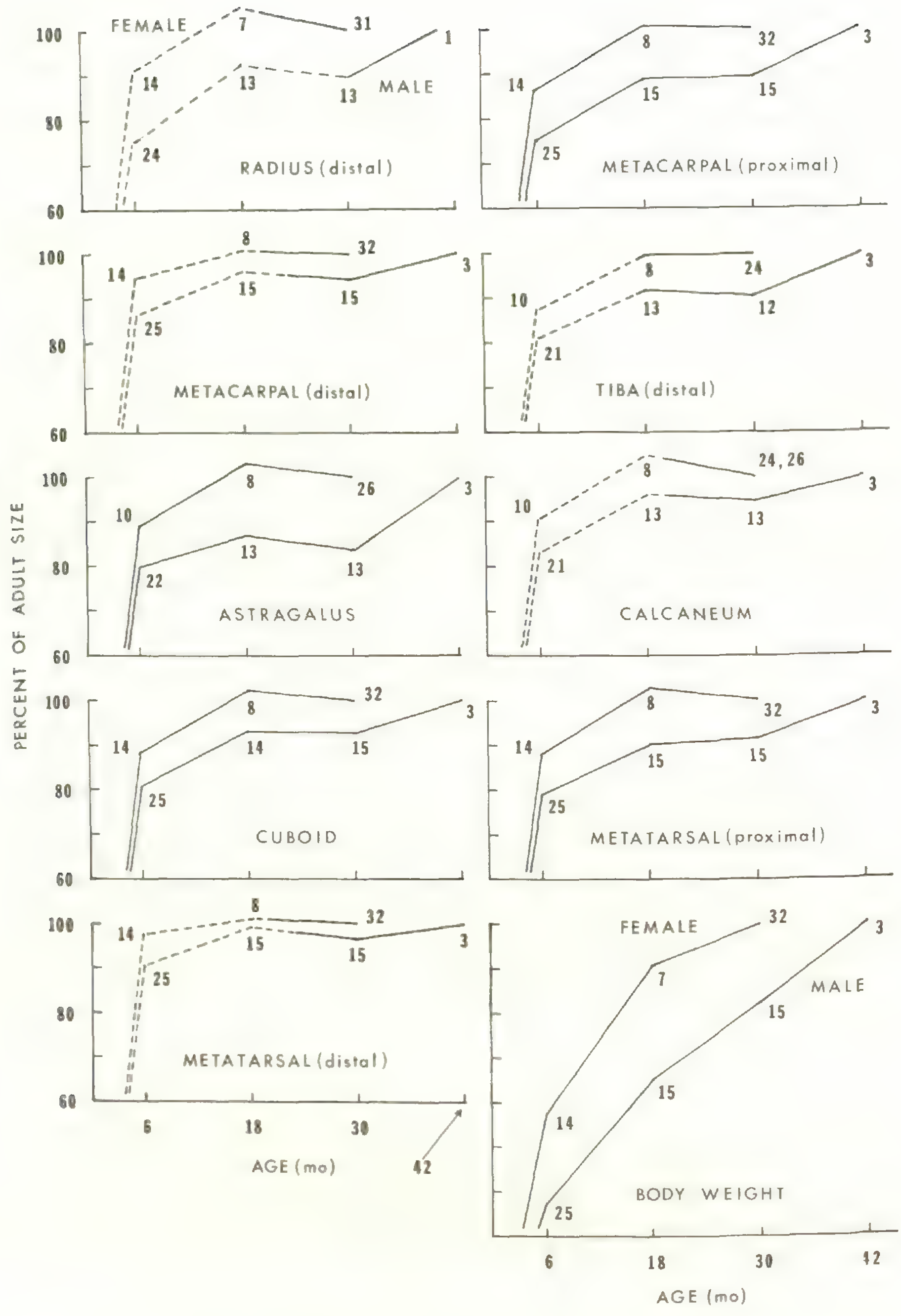

FIG. 2.—Growth of the leg bones and body weight of white-tailed deer from the Savannah River Plant, South Carolina. Females are represented by the upper curve on each graph. The dashed portion of a curve indicates unfused epiphyseal plates (Purdue 1983a). Percentages are based on cross-sectional area for the radius (distal end), metacarpal (proximal end), tibia (distal end), calcaneum, and metatarsal (proximal end); on volume for the astragalus and cuboid; on one linear measurement for the metacarpal (distal end) and metatarsal (distal end); and on kg for body weight. Sample sizes are indicated by each point (dual sample sizes mean one or more specimens could not be measured for one variable).

TABLE 3.—*Regression analyses testing the effect of locality on the bone size-body weight relationship. Adult females from 5 localities (Series 1: Ossabaw Island, Georgia [OS]; Savannah River Plant, South Carolina [SC]; Pope County, Illinois [PO]; and Macon [MA] and Hickory [HI] counties, Missouri) or from 4 localities (Series 2: exclude Ossabaw Island) were used. On each series of localities, two analyses per measurement set were performed: the first tested for the homogeneity of slopes. Only probabilities are reported. If not significant at the 0.05 level, a second analysis examined homogeneity of Y-intercepts (critical probability <0.05). Significant probabilities are indicated by a *. Also indicated are pairs of localities that showed significant differences in adjusted means. Abbreviations are given in Table 2.*

Measurement Set	Series	Probability Slope	Y-intercept	Significant Pairs
RDAR	1	0.73	0.00*	OS-PO; OS-SC
	2	0.47	0.60	
TIAR	1	0.31	0.00*	OS-PO; OS-MA; OS-HI; OS-SC; HI-SC
	2	0.66	0.02*	MA-SC
MCPAR	1	0.25	0.00*	OS-PO; OS-MA; OS-SC
	2	0.50	0.06	
MCDW	1	0.74	0.00*	OS-PO; OS-MA; OS-HI; OS-SC
	2	0.59	0.14	
MTPAR	1	0.16	0.00*	OS-PO; OS-MA; OS-SC
	2	0.37	0.03*	MA-SC
MTDW	1	0.08	0.01*	OS-MA; OS-SC
	2	0.27	0.08	
ASVO	1	0.09	0.00*	OS-PO; OS-MA; OS-HI; OS-SC
	2	0.46	0.06	
CUVO	1	0.74	0.00*	OS-PO; OS-MA; OS-HI; OS-SC
	2	0.75	0.04*	MA-SC
CAAR	1	0.18	0.01*	OS-PO; OS-MA' OS-HI; OS-SC
	2	0.18	0.09	

South Carolina (Table 6). Admittedly, the latter deer were part of the sample upon which the regressions were based, but, since independent specimens of known weight were lacking, the use of the Savannah deer must suffice. The sex-specific regressions applied to the South Carolina deer successfully estimated actual body weight. Also, the regressions consistently indicated that the Illinois deer were significantly larger than those from South Carolina. Finally, the combined-sex regressions for Jo Daviess County deer yielded slightly higher, significant weight estimates when compared to sex-specific values.

TABLE 4.—*Regression analyses testing the effect of sex on the bone size-body weight relationship. Two analyses per measurement set were performed: the first tested for the homogeneity of slopes. Only probabilities are reported. If not significant at the 0.05 level, a second analysis examined homogeneity of Y-intercepts (critical probability <0.05). Significant probabilities are indicated by a *. Abbreviations are given in Table 2.*

Measurement Set	Probability Slope	Y-intercept
RDAR	0.45	0.02*
TIAR	0.56	0.00*
MCPAR	0.59	0.00*
MCDW	0.27	0.07
MTPAR	0.60	0.00*
MTDW	0.27	0.00*
ASVO	0.15	0.00*
CUVO	0.60	0.00*
CAAR	0.48	0.00*

DISCUSSION

The weight of a deer at any particular time is the net result of many factors, some of which can and some of which cannot be controlled in archaeological contexts. Transient changes in weight, such as seasonal shifts in fat deposits and year-to-year variation, cannot be addressed given the resolution presently derivable from archaeological data. For instance, the amount of stored fat can fluctuate faster (Moen and Severinghaus 1981) than most seasonal indicators are able to detect change (Morey 1983). Body weight can vary up to ±30% annually due to accumulation and depletion of fat stores, as dictated by forage conditions, weather severity, photoperiod, and other physiological fluctuations, e.g., pregnancy (Moen and Severinghaus 1981). Similarly, year-to-year variation in body weight of deer of like sex and age, sampled in late fall, can vary significantly (Kirkpatrick *et al.* 1976). Changes such as these leave no markers in bone remains, but even if they did, the temporal control of virtually all archaeological sites is insufficient to make sense of the resultant patterns. Although transparent to us through studies on bone, these transient factors could have been critical for prehistoric deer hunters.

In contrast to transient fluctuations in weight, bones of modern deer can reflect the effects of sex and ontogenetic age on body weight, but the picture is more complicated than indicated in previous studies (e.g., Emerson 1983). Differential growth of body parts, particularly the rapid development of lower leg bones relative to the slow accumulation of body weight, makes the estimation of weight for young deer inaccurate, as was also noted, but not fully explored, by Emerson (1978). Estimates based on elements with fused epiphyseal plates are more trustworthy, but even here, body weight often continues to increase after the time of fusion. Realistic estimates of weight are further complicated by the sexes differing in their relationships between bone size and body weight. Unfortunately, post-cranial deer bones from archaeological sites can only rarely be assigned sex and ontogenetic age (but, for an exception, see Purdue 1983b).

The reference sample of deer in the current study was diverse, covering portions of five states. Fortunately, once the Ossabaw Island specimens were deleted, no con-

TABLE 5.—*Regressions for estimating body weight from bone size of white-tailed deer. Separate equations are presented for females (≥30 mo.), for males (≥42 mo.), and for combined sexes (males and females ≥42 mo.). See Table 2 for measurement set abbreviations; probability abbreviations are: ns, non significant; **, P 0.01; and ***, P 0.001.*

Measurement Set	Sex	In A	B	N	s_y	r^2	F
RDAR	M	-2.97198	1.07685	11	0.07705	0.83	45.1***
	F	-1.92477	0.90382	95	0.10543	0.56	117.6***
	Both	-3.34697	1.12422	11,35	0.11132	0.74	122.9***
TIAR	M	-5.90302	1.52647	13	0.09687	0.76	32.2***
	F	-4.80119	1.34048	78	0.10485	0.60	112.7***
	Both	-7.23944	1.72084	13,31	0.10888	0.79	156.4***
MCPAR	M	-3.39005	1.22741	13	0.09672	0.75	32.4***
	F	-2.65716	1.08655	97	0.10794	0.53	108.1***
	Both	-4.68886	1.42989	13,36	0.12336	0.70	111.5***
MCDW	M	-5.11833	2.71314	13	0.08827	0.79	41.1***
	F	-3.23203	2.14051	97	0.10094	0.59	137.2***
	Both	-4.46664	2.51464	13,36	0.11734	0.73	128.2***
MTPAR	M	-5.27456	1.48019	13	0.08622	0.82	48.7***
	F	-4.46255	1.32736	87	0.11124	.058	116.5***
	Both	-6.69049	1.68850	13,33	0.12777	0.73	120.5***
MTDW	M	-5.52915	2.81534	13	0.09660	0.77	36.6***
	F	-3.52154	2.19842	97	0.11069	0.56	123.1***
	Both	-5.11749	2.68206	13,36	0.13243	0.70	109.6***
AVSO	M	-8.13984	1.26867	13	0.06957	0.88	80.8***
	F	-4.62775	0.88870	80	0.12435	0.50	76.6***
	Both	-9.49655	1.40109	13,31	0.11862	0.78	148.6***
CUVO	M	-3.54197	0.87090	13	0.13959	0.49	10.6**
	F	-2.55390	0.73732	86	0.12306	0.45	70.1***
	Both	-5.05259	1.03017	13,33	0.14936	0.59	63.2***
CAAR	M	-3.33992	0.98336	11	0.15156	0.31	4.1ns
	F	-5.75054	1.27511	76	0.11429	0.54	85.9***
	Both	-8.40131	1.63313	11,30	0.14471	0.63	65.8***

sistent difference in the bone size-body weight relationship was noted, thus moderating Emerson's (1983) concern that modern deer outside of south-central Wisconsin, the origin of his sample, might show a different bone-weight relationship. However, clinal variation through time, which is now known to affect deer (Purdue 1986), impacts the utility of Smith's (1975) method for estimating body weight. Smith suggested equating weights of like sex and age classes of archaeological and modern deer from the same locality. Given the dynamic nature of deer size (Purdue 1986), the ability to choose a suitable modern analog is diminished.

TABLE 6.—*Comparison of actual adult female weight and that estimated from bone size. The samples originated from Jo Daviess County, Illinois (N = 24), and the Savannah River Plant, South Carolina (N = 24). The weight was estimated for each individual deer, then descriptive statistics were calculated (the mean in kg and the standard deviation in parentheses are shown). Separate estimates were made using each bone element and regression model (Table 5). See Table 2 for abbreviations.*

Measurement Set	South Carolina Sex-specific	Jo Daviess Co., Illinois Sex-specific	Jo Daviess Co., Illinois Combined-sex
RDAR	43.06 (2.75)	59.36 (3.38)	62.01 (4.40)
TIAR	45.30 (3.99)	60.41 (5.04)	66.41 (7.00)
MCPAR	43.78 (4.03)	57.38 (4.20)	62.69 (6.00)
MCDW	44.14 (3.45)	59.28 (4.77)	61.79 (5.80)
MTPAR	45.29 (4.64)	58.07 (4.48)	63.68 (6.23)
MTDW	45.05 (3.78)	60.10 (5.23)	65.17 (6.89)
ASVO	44.44 (4.15)	57.07 (6.06)	65.39 (11.16)
CUVO	45.29 (4.09)	56.92 (4.71)	65.24 (8.26)
CAAR	45.51 (3.66)	58.34 (4.42)	64.90 (6.33)
Actual Body Weight	44.60 (6.38)		

Summary of analyses of variance:

1. A one-way anova showed no significant differences in the series from South Carolina that included estimates from different measurement sets and actual body weight.
2. A two-way anova indicated significant differences between sex-specific estimates between South Carolina and Jo Daviess Co., Illinois, but no significant differences among measurement sets or the interaction term.
3. A two-way anova indicated significant differences between sex-specific and combined-sex estimates for Jo Daviess Co., Illinois, but no differences among measurement sets or the interaction term.

In spite of numerous limitations, estimates of the body weight of prey species contributes an added perspective to the interpretation of human subsistence. For example, measurements of astragali of deer from central Illinois indicated size shifts through time have occurred (Purdue 1986). When these measurements are converted to body weight, a sense of the magnitude of the difference between time periods can be achieved. Late Archaic inhabitants of the Pabst Site (DeWitt County) took adult bucks and does that averaged 77.7 and 52.2 kg., respectively. Nearby, but 3,000 years later, residents of the Crable Site (Fulton County) hunted bucks that weighed 102.0 kg. and does that weighed 60.4 kg. The resultant difference in yield (24% for males and 14% for females) suggests that the size factor within a prey species could be a consideration in human procurement strategies.

It is imprudent to consider any archaeological bone-based estimate of body weight reflective of reality. Rather, an estimate should be viewed as an index that smooths multiple compounding factors and is useful only in an ordinal sense, not unlike that suggested by Grayson (1984) in a slightly different context. The present study attempted to isolate and to evaluate the relative impact of the various compounding factors so that

the limits of body weight estimates would be better known. In that context, use of either Emerson's (1978, 1983) regression based on astragalus length or the set of equations presented in this study is satisfactory, although the latter system has certain advantages:

1. Estimates of weight are based on more than one measurement on an element, which, in most cases, compensate for vagaries that can affect a single measurement.
2. Better control and greater sample size were maintained for the reference deer.
3. Log-log transformations of original data improved the fit of linear regressions (e.g., Gingerich *et al.* 1982).
4. Weight estimates are possible based on any one of nine measurement sets representing seven elements in a unified system that yields consistent results. Thus, weights of deer from different sites, or from within the same site, can be compared, even though the estimates may be based on different bones.
5. Possible effects of geographic variation were taken into account.

Other methods, namely White's (1953) and Smith's (1975), although important contributions in their time, probably should be no longer used.

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MEDICINAL ETHNOBOTANY OF HMONG REFUGEES IN THAILAND

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ABSTRACT.—Every society has a healing system and many groups use medicinal herbs. The Hmong refugees of Southeast Asia possess a knowledge of herbalism that is as yet unknown to the American practitioners who now treat these people. Indeed, Westerners have been suspicious, suggesting that traditional healing arts are a health risk factor. This study attempts to describe Hmong herbal knowledge by identifying plants and their usage. Nine herbalists in a refugee camp were asked to identify plants they use for medicine. Interviews generated more than 150 plant references. Discussion centers around 20 plants used by more than one herbalist. Further documentation and collection of voucher specimens, medicinal preparation procedures, and native concepts of disease and illness promises to be of practical value in discovering both new sources of drugs and inexpensive herbal preparations, in providing health care to the Hmong, and in preserving the herbal lore of a rapidly acculturating group.

INTRODUCTION

The Hmong are one of several groups of Southeast Asians who, during the past decade, have fled their native homeland for temporary asylum in Thailand. The Hmong of Laos, along with other highland Laotians, are notable because they have lived in a remote region and have remained largely dependent upon their indigenous healers for medicine. While acculturative forces affecting the Hmong have gathered momentum in concert with recent political changes, among the older Hmong, especially, medicinal herbal knowledge is still intact and actively practiced. Working in refugee camps with the Hmong, Western health personnel have been intrigued by the Hmong community's tenacity to their indigenous medicine as well as their low utilization of the available Western medical services. In comparison to refugees of other ethnic groups, the Hmong have the lowest per capita rate of visits to Western clinics (unpublished data, COERR Hospital Log, Phanatnikhom).

Since the Hmong language was not written until about 35 years ago when French missionaries created a script (Bliatout 1982:2), legends, medicinal knowledge, and stories have been passed down for centuries through the oral tradition (Barney 1983:37). Those most knowledgable in the cultural lore are the elder members of the community who, for the first time, received Hmong literacy training in refugee camps as part of a required orientation allowing entrance to the United States or other countries of resettlement. Because of the massive acculturative pressures experienced by this group as they are uprooted from their homeland and transferred to a new continent, as with many isolated indigenous cultures, there is now considerable danger of loss of transmission of medicinal plant lore to the youth of the society. If such knowledge is not soon recorded, not only will it be lost forever, but its potential as a source of new prototype drugs and of researchable low cost medicines for third world populations will not be realized (Elizabetsky 1986).

The suggestion that the Hmong pharmocopoeia contains agents related to diseases—e.g., heavy metal poisoning and sudden unexplained death syndrome (SUDS)—creates further impetus for this study. Several Hmong home remedies (not of herbal origin) recently have been found to be high in lead and arsenic content (Centers for Disease Control 1983:555), but a definitive link between the use of herbal medicines and heavy metal poisonings in the Hmong community has not been established.

The victims of SUDS are predominantly healthy 25-44 year old Asian males who die, often at night after falling asleep. They reportedly suffer labored breathing, screams, or tossing in bed immediately before death (Centers for Disease Control 1981:581). Since the greatest rate of the syndrome among Southeast Asians occurs in the Hmong population, epidemiologists are therefore considering those agents to which the Hmong population is exposed, including traditional medicines (Holtan *et al.* 1984:106).

Several anthropological and medical anthropological works on the Hmong are now available (Geddes 1976; Chindarsi 1976; Lemoine 1972; Vidal 1969; Kundstadter 1985). Lemoine and Vidal (1970) jointly published over 300 ethnobotanical uses, both medicinal and otherwise, of Hmong flora. The purpose of the current study was to continue the identification of Hmong herbal medicines and their uses in a setting where Western medicine is being freely offered but not well received. The high utilization of their traditional healing arts makes current documentation of Hmong herbalism both timely, in terms of preserving cultural knowledge, and potentially rewarding, in terms of finding new and effective medicinal plants.

HMONG HISTORY, CULTURE, AND HEALING

The first recorded history of the Hmong, who are of Sino-Tibetan origin, is found in Chinese literature around 2250 B.C. The Chinese frequently advanced militarily against the Hmong, beginning in the 12th century A.D., which has been a continual stimulus for their migration southward. Through centuries of oppression, the Hmong have demonstrated a love of independence and an ability to organize for military action (LeBar *et al.* 1964:63.) "Hmong", which means "free man" is their own term for themselves, while their Chinese and Thai neighbors may call them "Meo" or "Maio" (Bliatout 1982:2). They arrived in Southeast Asia in the early 1800s and today occupy parts of southern China, Laos, Vietnam, and Thailand with an estimated population of 2,600,000 (Bliatout 1982:2; Chindarsi 1976:1).

During the Vietnam era, many Hmong who feared communist aggression in Laos furnished military intelligence and guerilla support to the American troops who supported the Royal Lao Government. When the U.S. withdrew in 1973, the Royal Lao Government fell. Those who opposed the communists fled and the first of several waves of emigration occurred. Over 100,000 Laotion Hmong have found political refuge in Thailand and perhaps an equal number have died in flight (Garrett 1980:633). Many Hmong have spent years in refugee camps waiting to be resettled or until it is safe to return to their homeland. Over 50,000 others, in return for the support that the hilltribe gave to American military efforts, have been granted permanent asylum in the United States (Hammond, 1984).

Historically, the Hmong farm mountain slopes at elevations over 1600m where they practice slash and burn agriculture, growing rice and corn and raising chickens and pigs for their subsistence. The opium poppy was introduced to the area by the British East Indies Company in the late 1700s, at which time the Hmong started to grow it as a cash crop and for medicinal purposes (Yang 1982:4). Cultivation of opium was legal under the French colonial domination. The harvest was used first for income, for which they traded with the Yunnanese merchants of Chinese descent. Opium was used medici-

nally as a painkiller—especially for the elderly—and its use by healthy young people was considered a disgrace (Koumarn 1983:9).

The Hmong of Laos have more than 20 clans which are patrilineal and exogamous (Yang 1982:3). Among Thai Hmong, villages comprise 30 to 50 households (Kunstadter 1983:330). Leadership is characteristically heirachical but not dictatorial (Minnesota Refugee Resettlement Office 1980:6). The Hmong Njua (Blue or Green Hmong) and the Hmong Da (White Hmong) are the two major dialects of the Hmong language and the members of different dialects sometimes intermarry (Kunstadter 1983:330).

When a Hmong person becomes sick, the family first employs the knowledge of the household in terms of herbs, massage, simple rituals, or soul-calling practices. If the illness is sufficiently grave, an elder of the household may perform a divining ritual to determine which outside health practitioner is the most appropriate to consult. This may be an advanced herbalist, shaman, or other who has the needed skills (Bliatout 1984:34-37; DeSilva 1984:4A).

The Hmong believe in both natural and supernatural explanations of disease causation (Geddes 1976:97). Natural explanations refer to etiologies such as eating rotten food or traumatic accidents. From this concept arises the body of knowledge about medical measures aimed at specific symptoms, for example, indigenous herbs, massage, bone-setting, and acupuncture. Supernatural explanations of disease, in contrast, refer to the misfortune caused by angered or needy spirits or a "wandering soul" (Tung 1980). The Hmong generally attribute their healing successes to the powers of the spirits (Chindarsi 1976:100). To treat diseases of spirit cause, the Hmong employ a spiritual healer, the most powerful of which is the shaman. The shaman may be male or female and usually obtains a diagnosis while in a trance state and communicating directly with the spirit world. A sacrificial ceremony is often prescribed. He or she may also have skills in the secular practices including herbalism (Bliatout 1984:36; Chindarsi 1976:41).

PROCEDURES

This research took place in Phanatnikhom refugee camp, 90 km southeast of Bangkok, where I worked as a public health nurse for the six months prior to the study. This is an inland camp to which refugees from Laos, as well as Cambodia and Vietnam, are transported for "processing" before being allowed to depart to a third country of permanent asylum.

Hmong community leaders suggested informants/consultants for participation in the study. With the assistance of an interpreter, four male and five female refugees, ages 46-74, were asked to name medicinal plants and report indications for use and method of administration. Their personal collections of medicinal herbs in this lowland refugee camp were precious and few (Fig. 1) because many of the plants had been carried on the long hot journey from the northern highlands camp.

Although I was aware of the importance of voucher specimens in ethnobotanical data collection—an awareness now deepened by Bye's (1986) cogent treatment—I nonetheless found myself in a dilemma: to insist upon voucher specimens or not, realizing that the informant would zealously guard each individual plant much as would a naturalist want to protect an individual of an endangered species. The refugee camp setting heightened the ethical dilemmas always inherent in cross-cultural data collection. Obtaining an informed consent from a member of a non-literate culture is an ethical dilemma in itself. Researchers must strive to obtain true community understanding and support of a project, and to make every effort to return the results of the study, in understandable form, to those who contributed most to its availability.

FIG. 1.—A typical Hmong garden of medicinal herbs.

The informants, displaced from their homeland, threatened by a sense of cultural loss, insecure about their chance for eventual resettlement and a return to freedom, were politically powerless and eager to make American contacts. In this setting, the researcher is a representative of a highly technological, poorly understood, encroaching and powerful culture touting one of the western world's more sophisticated concepts. In light of this situation, rather than risk unethical practice or ill-will, which would jeopardize the reliability of the information, I chose to limit voucher specimens to what was freely offered and to supplement this meager record with photographs. The seeds and barks which were given to me were often not suitable for definitive identification. Yet the trust engendered by non-invasively collecting data and by returning the information in understandable form to the Hmong people would most serve the aim of the project, to promote good will and understanding between the Hmong community and the Western scientific community.

Garden specimens, dried specimens, and photographs were used during the interviews. Scientific names given are those "assigned" by the herbalists when they recognized photographic plates form Chinese Medicinal Herbs of Hong Kong (Li and Cheung, 1978, 1980, 1982). Family names were obtained from Perry (1980), with one exception, *Stahlianthus involucratus*, which was obtained from Hutchinson (1973).

The focus of this report is on those plant medicines referred to independently by two separate herbalists. Medicines used consistently by different herbalists represent those with the greatest likelihood of being pharmacologically active (Trotter 1983). The population of a refugee camp is derived from many different communities, thus is it conceivable that the uses reported by two different herbalists are widespread among the

Hmong. It was considered that two herbalists were referring to the same plant if one of the following occurred: (a) both herbalists identified the same photograph by Hmong names that did not conflict (although sometimes no Hmong name was known by one of the herbalists) or (b) they used the same Hmong name when referring to a plant medicine that was not pictured in the books or (c) they both provided the researcher with plant parts sufficient to conclude that they were referring to an identical species.

FINDINGS

Of over 150 plant references obtained, 20 plants were used to two of the nine herbalists. Of these, eight were used for identical or similar indications. The findings are summarized in Table 1. Those plants without tentative botanical nomenclature are listed by their Hmong names in part 2 of Table 1. Each of the nine herbalists reported at least one medicinal plant mentioned by another interviewed herbalist.

In some instances, the Hmong indication was not readily translatable into a Western counterpart. In such cases, the Hmong word for the indication has been included in the reported data. Neither the pharmacopoeia nor the translations of perceived illnesses are considered complete.

CONCLUSION

The Hmong person, forced between the extremes of two cultures, must both learn to adapt and meet the challenges of preserving cultural identity. Medicinal plants are a major part of their rich heritage. It is unfortunate that, at times, Hmong persons clash with conventional Western practitioners over the use of medicinal plants. Unless a specific medicine is known to be harmful, the Western practitioner should not interfere with its use. Herbal knowledge handed down for centuries through the oral tradition likely has merits. Some of these plant medicines may have valuable uses; others may be related to the Hmong's experience of Sudden Unexpected Death Syndrome (SUDS), of heavy metal poisoning, or be otherwise toxic. This study attempts to describe Hmong herbal knowledge by identifying plants and their usage. Further documentation and collection of voucher specimens, medicinal preparation procedures, and native concepts of disease and illness promises to be of practical value in discovering both new sources of drugs and inexpensive herbal preparations, in providing health care to the Hmong, and in preserving the herbal lore of a rapidly acculturating group. The scientific community should support efforts to further investigate the nature of these herbs. Hmong leaders and individuals should be made aware of the benefits of such research and should be encouraged in taking an active role in preserving the knowledge and determining the pharmacologic and toxic properties of these herbs.

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TABLE 1.—*Medicinal Plants Used by More Than One Herbalist.*

Scientific name Hmong name Family	Part Used	Preparation	Indication(s)	Method of Use
Aloe vera L. var. *chinensis* (Haw.) Berg.	(1) liquid from leaf		(1) burns	(1) topically
	(2) liquid from leaf		(2) prevent rabies dogbite	(2) topically
Tshuaj Kub Cev or ***Po Nyuj***	(3) leaf	(3) cold infusion	(3) high fever with convulsions	(3) orally
	(4) leaf	(4) cold infusion	(4) weak limbs after convulsions	(4) orally
Liliaceae				
Curcuma zedoaria (Berg.) Rosc.	(1) root (bulb)	(1) chop very fine; mix with water	(1) coma coming on as death approaches ***(Mob Yuas)***	(1) orally
Qhiav Dub	(2) root (bulb)	(2) chopped; use with root of ***Wam Khoo;*** infusion may be made	(2) dysentery	(2) eat roots or drink infusion
Zingiberaceae				
Equisetum debile Roxb.	(1) stems	(1) decoction; mixed with root of ***Suab Xov Tooj*** *(Lygopodium japonicum* (Thunb.) Sw.)	(1) painful, difficult urination; kidney stones ***(Mob Txeeb Zig)***	(1) orally
Rag Deg	(2) stems	(2) crushed until soft	(2) broken bone (Lov Txha)	(2) applied topically
Equisitaceae				

TABLE 1.—*Medicinal Plants Used by More Than One Herbalist.* (continued)

Scientific name Hmong name Family	Part Used	Preparation	Indication(s)	Method of Use
Euphorbia antiquorum L.[1] ***Xeeb Leej Tsav*** Euphorbiaceae	latex from stem; leaf	cut stem to draw latex; boil with chicken and papaya; sometimes used with *Euphorbia hirta* L.	increase flow and amount of breast milk[2] ***(Zoo Poj Niam Muaj Mis)***	orally
Jatropha curcas L. ***Txiv Thooj Ywg*** Euphorbiaceae	(1) latex from stem (2) fruit	(1) cut stem to draw fluids (2) crush until soft; put the oil from the fruit in the house for the spirit to eat; used with *Sapium discolor* Muell.-Arg. and *Sapium sebiferum* Roxb. (all three species have same Hmong name.)	(1) oral thrush (contraindication: measles (2) to kill ***Dab Ntxaug*** (a greatly feared, powerful spirit, often blamed for epidemics)	(1) apply to oral mucous membranes (2) not consumed by humans
Lygopodium japonicum (Thunb.) Sw. ***Suab Xov Tooj*** Schizaceae	(1) root (2) root	(1) decoction; boiled with chicken. (2) decoction; mixed with root of ***Rag Dev*** *(Equisetum debile* Roxb.)	(1) weakness (2) painful, difficult urination; kidney stones ***(Mob Txeeb Zig)***	(1) orally (2) orally

TABLE 1.—*Medicinal Plants Used by More Than One Herbalist.* (continued)

Scientific name Hmong name Family	Part Used	Preparation	Indication(s)	Method of Use
Mirabilis jalapa L.[1] Kuab Tub Sab or Paj Kuab Tub Sab Myctaginaceae	(1) root (for treating man, should **not** be forked) (2) (for woman, **should** be forked.	(1) about 1 tsp. finely chopped; decoction made by boiling with chicken or meat. (2) (about 1 tsp.) finely chopped; mix with chicken or meat.	(1) man: weak, impotent, problem urinating[2] ***(Txiv Neej Qhuav Cev)*** (2) promote delivery of blood clot after birth	(1) orally (2) orally
Ocimum basilicum L. ***Zaub Txig Theem*** Libiateae	(1) leaf (2) seed	(1) mix with ***Zaub Txib Taum Paj,*** egg yolk, and sulfur; fry in pork, goat, and chicken oil. (2) —	(1) scabies (2) foreign object in eye	(1) apply topically (2) apply inside conjunctivae as if applying eye ointment (seed and foreign object migrate to corner of eye and are easy to remove.

TABLE 1.—*Medicinal Plants Used by More Than One Herbalist.* (continued)

Scientific name Hmong name Family	Part Used	Preparation	Indication(s)	Method of Use
Opuntia dillenii (Ker-Gawl.) Haw.	(1) cactus pad	(1) crush until soft	(1) big spleen	(1) apply topically over spleen
Xeeb Leej Tsav or ***Xeeb Leej Tsav Loj***	(2) pad	(2)*a* take 3-4 "cuts"; chop finely; mix with egg to eat, or (2)*b* heat pad over fire; apply over liver.	(2) big liver	(2) orally or topically
Cactaceae				
Pinus massoniana Lamb.	(1) pollen	(1) cooked with food	(1) leproxy-like disease ***(Mob Qhe)***	(1) orally
Ntoo Thuv	(2) sap	(2) mix with ashes	(2) scabies	(2) apply topically
Pinaceae				
Piper hancei Maxim.	(1) root	(1) decoction	(1) prolapse uterus ***(Hlauv Duav)***	(1) wash uterus and vagina
Hmab Saw Nyiaj	(2) NA[3]	(2) NA[3]	(2) gonorrhea	(2) NA[3]
Piperaceae	(3) leaf and root	(3) decoction; boiled with chicken; used with two other medicines.	(3) "arthritis" (Mob Laug)	(3) orally

TABLE 1.—*Medicinal Plants Used by More Than One Herbalist.* (continued)

Scientific name Hmong name Family	Part Used	Preparation	Indication(s)	Method of Use
Ranunculus cantoniensis D.C.[1] ***Tshab Xyoob*** Ranunculaceae	(1) root	(1) use with **Muaj Tsuas** *(R. scleratus)* and ***Hmab Laum Liab.*** Take 5-10 cuts of the roots. Prepare a decoction with or without chicken.	(1) man: weak, impotent, problems urinating	(1) orally
	(2) root	(2) use small amount. Mix with ***Muas Tsuas*** *R. scleratus* L.), blood of mountain goat, ***Sab Txim Dub;*** cook with with chicken.	(2) "arthritis"[2] ***(Mob Laug)***	(2) orally
	(3) root	(3) decoction	(3) poor vision	(3) orally
Ranunculus scleratus L.[1] ***Muaj Tsuas*** Ranunculaceae	(1) NA[3]	(1) as for *R. cantoniensis* D.C.	(1) arthritis ***(Mob Laug)***	(1) orally
	(2) root	(2) as for *R. cantoniensis* D.C.	(2) man: weak, impotent, problems urinating ***(Triv Neej Qhuar Cev)*** ***(Triv Neej Qaug Zog)***	(2) orally

TABLE 1.—*Medicinal Plants Used by More Than One Herbalist.* (continued)

Scientific name Hmong name Family	Part Used	Preparation	Indication(s)	Method of Use
Stahlianthus involucratus (King ex Bak.) Craib.[1]	(1)*a* leaf (1)*b* root	(1)*a* decoction; boiled with chicken (1)*b* NA[3]	(1) stomach ache after eating bad food ***(Mob Kem Plab)***	(1) orally
Tsawb Ntug Ntsuab "green banana tree" Zingiberaceae	(2) root	(2) hot infusion	(2) hot inside ("from eating something wrong")	(2) orally
Stephania tetrandra S. Moore[1]	(1) root	(1) finely chopped	(1) stomach ache after eating bad food ***(Mob Kem Plab)***	(1) orally
Hmab Cab Qaib Menispermaceae	(2) root	(2) use 10 or more "cuts" finely chopped. May be used with 5 "cuts" of Hmab Laum Liab *(Tinospora saggita* Gagn.). Infusion.	(2) stomach ache and intestine pain with hard stool	(2) orally
—	(1) NA[3]	(1) used 3 "cuts"; chop finely; mix with egg; toxic in high doses. (Comes from China.)	(1) chronic cough with noisy breath sounds[2] (tuberculosis)	(1) NA[3]
Kuab Ib or ***Kuab Ib Suav***[1]				

TABLE 1.—*Medicinal Plants Used by More Than One Herbalist.* (continued)

Scientific name Hmong name Family	Part Used	Preparation	Indication(s)	Method of Use
— ***Nplaig Dev Ntsuab***[1]	(1) root	(1) dried and shredded; decoction; cooked with chicken or meat with fat removed.	(1) anemia[2]	(1) orally: 1 tsp. weekly
	(2) root	(2)*a* dried and shredded. (2)*b* sometimes prepared as hot infusion.	(2) gonorrhea[4] (both kinds)	(2) orally: (a) 1 tsp. 2 times daily until better (b) drunk as tea.
— ***Thoob Huab***[1]	root	decoction	constipation[2]	orally
— ***Txiv Quav Nees***[1] "seed of horse stool" (the seed smells like stool)	(1) pod-like fruit, when it is dark, moist and sweet tasting inside	(1) hot infusion of sweet insides	(1) dysentery	(1) orally
	(2) seed within pod-like fruit when it is dry and white inside	(2) powdered; make infusion or take as is	(2) constipation[2]	(2) orally
— ***Txiv Taum Ntswj*** ***(Ntswj*** = "to twist, to writhe")	(1) fruit. If lines spiral to left, use for women. If lines spiral to the right, use to treat men.	(1) use one whole fruit; hot infusion.	(1) moving stomach pain[2] sometimes with diarrhea and muscle spasms in arms and legs ***Mob Plab Ntswj)***	(1) orally

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NOTES

[1]Current address: Department of Ecology and Evolutionary Biology, University of Arizona, Tucson, AZ 85721.

[2]This plant used by herbalists for identical indication (denoted 2) or closely related indications.

[3]Indication treated by two herbalists using this plant.

[4]Information not available.

[5]There are two kinds of gonorrhea called ***Mob Kas Cees.***
(a) penis painful with pussy discharge, sometimes accompanied by blister on penis.
(b) no puss, no blister; low stomach and back pain, painful urination; dark yellow urine; pain increases with beef or alcohol consumption.

[6]Heimbach, 1979:28.

CRC Handbook of Proximate Analysis Tables of Higher Plants. James A. Duke & Alan A. Atchley. Boca Raton, FL: CRC Press, 1986. Pp. 400. $100.00 (cloth).

This new reference book from CRC contains tabulated information on the nutritional composition of hundreds of species of vascular plants, compiled from some 22 previously published sources. Information on calcium, phosphorous, iron, sodium, potassium, carotene, thiamin, riboflavin, niacin, and ascorbic acid is included, where these values are reported in the original literature, in addition to water, protein, carbohydrate, fiber, and ash. The authors state in the introduction that the information is also available on computer tape and that they are planning yearly updates of the book.

Two main tables comprise most of the book, both reproduced directly from computer printouts. The first contains data "directly transcribed from original sources," while the second contains the same information "converted to a zero-moisture basis." The authors make a good case for converting the data to a dry weight format; unfortunately, much of the information in the first table is also reported in the same way, as evidenced by the potentially misleading zeroes listed for percent water. Many references which report figures on a dry weight basis do have values for percent water since this can be a valuable piece of information in practical applications. Changing this to a zero detracts from the value of the table. Perhaps one table for fresh weight and one for dry weight would have been a more useful organizing scheme.

The printout is easy to read for the most part (although a few of the pages are miscollated), except that the table is broken into two parts on some pages but not on others. Units and coding information are presented only in the introduction, rather than alongside the numerical data. Only a few of the samples give any indication of preparation techniques, which can be important since cooking, drying, or other processes can significantly change food composition (Kuhnlein 1986).

Many of the more important plants utilized by native peoples around the globe are included, plus many species of lesser importance, but significant gaps do exist. Some of these reflect lacunae in the literature on the subject of nutritional composition, but others are oversights due to the short lists of references cited. Most of the information comes from extensive compilations previously published by the USDA and the FAO and in the pages of *Economic Botany*. Little effort seems to have been made to track down less obvious sources of information. They have especially relied on the USDA seed screening project from several years ago (Earle & Jones 1962; Jones (Earle 1966; Barclay & Earle 1974). The results of this project have been criticized since the protein values reported are consistently higher than those found by other workers studying the same species (Nabhan *et al.* 1979). One of the other references (Hilty *et al.* 1972) is an abridged, popularized version of an article more readily available elsewhere (Benson *et al.* 1973).

There is a potential error in the authors' methodology concerning the values presented for carotene, although I am unsure whether they have fallen into the trap they have set for themselves. They state that the figures for carotene were obtained by doubling the published values for vitamin A. This is fine if the information was reported in SI units (milligrams, micrograms, etc.), or in the old International Units (IUs), but if the original reference presented or calculated the vitamin A values in terms of the newer standard unit, Retinol Equivalents (REs), Duke & Atchley's carotene figures will be too low by a factor of three (National Research Council 1980). I have seen articles employing REs but calling them "micrograms" or "microgram equivalents". Most of the references in this edition are too old to have used this new reporting technique, but if the authors add more information in the future following this line of reasoning they leave themselves open to error.

The book represents a good starting point for understanding the dietary contributions of traditional plant foods since it does contain a wealth of information. I hope the deficiencies will be corrected and the database expanded in the second edition.

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Economic Botany: Plants in Our World. B. B. Simpson & M. Conner-Ogorzaly. New York: McGraw-Hill, 1986. Pp. x, 640, illustr. n.p. (cloth).

There have been numerous textbooks in economic botany published in the last two decades, an indication of the proliferation of courses offered in this interdisciplinary field. Some have been aimed at courses for beginning students in non-science concentrations or majors, while others have been oriented towards students most of whom are enrolled in the sciences. This textbook, while striving to address both of these audiences, tends to be more technical than most of those recently published. It is to be recommended highly, especially for courses in the science fields; to take full advantage of what it offers the student should have a good grounding in biology and to have more than a beginner's familiarity with organic chemistry.

The book is divided into 19 chapters, each devoted for the most part to plant uses. Several of the chapters are especially interesting, perhaps because the contents are not found in most textbooks of economic botany: The Nature of Variation in Plants; Ornamental Plants; The Uses of Plants in the Future. Throughout the book ethnobotanical observations are freely and fully discussed. The index is highly detailed and consequently provides an excellent tool for the rest of the book. The choice of photographs is commendable, particularly because almost all are new. The many clear line drawings of plants—drawn by the second author—add much to the text.

One criticism might be offered: some of the chapters treat only a small fraction of major economic plants. For example, the discussion of waxes is reduced to two pages and treats only four plants, and only three sources of true rubber are mentioned, although the presentation of ***Hevea brasiliensis*** is noteworthy.

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Food, Diet and Population at Prehistoric Arroyo Hondo Pueblo, New Mexico. Wilma Wetterstrom. Arroyo Hondo Archaeological Series, No. 6. Santa Fe: School of American Research Press, 1986. Pp. xxi, 302. $15.00 (paper).

This ethnobotanical report—with contributions by Vorsila L. Bohrer on "Ethnobotanical Pollen" and R. W. Lang on "Artifacts of Woody Materials"—represents a large step forward in the fast-developing field of Southwestern archaeoethnobotany.

The book is divided into two parts: 1) the foods of Arroyo Hondo Pueblo, and 2) additional reports. The first part consists of sections on food plants, making a living in a marginal environment, lean times in a marginal environment, and diet, death, and demography.

Despite its geographical restriction, this volume has wide significance since it is one of the few that have recently appeared that view ethnobotanical or ethnoarchaeological information from an ecological aspect or from the point of view of the peoples who were alive and active at the time of the now extinct cultures considered. We hope for more contributions of this kind.

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THE ECONOMIC POTENTIAL OF *CHENOPODIUM BERLANDIERI* IN PREHISTORIC EASTERN NORTH AMERICA

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ABSTRACT.—A thin-testa domesticate chenopod *(Chenopodium berlandieri* ssp. *jonesianum)* was present in the eastern woodlands of North America by approximately 3500 BP., and this cultigen subsequently became one of the more important components of pre-maize plant husbandry systems in the East. The economic potential of this prehistoric domesticate can be projected through harvest studies of present day wild/weedy stands of *Chenopodium berlandieri.* During the fall of 1984 and 1985 a total of 86 plants located in nine states were harvested, with timed harvest hand stripping experiments carried out on 16 stands. Harvest yield values varied from 276 kg/ha to 2854 kg/ha. Harvest rate values ranging from .41 kg/hr to 1.6 kg/hr were recorded. Relatively conservative harvest yield and harvest rate values of 750-1500 kg/ha and 0.7 - 1.1 kg/hr are proposed for *C. berlandieri* in the prehistoric eastern woodlands, and when a seed coat thickness correction factor is applied, the resultant relatively conservative harvest yield range estimate of 500 - 100 kg/ha is found to favorably compare to quinoa and maize, as well as other eastern pre-maize cultigens.

INTRODUCTION
RESEARCH DESIGN

The tropical cultigen triad of corn, beans, and squash often is considered the core of prehistoric plant husbandry in Eastern North America, and these three crops were certainly of substantial economic importance during the late prehistoric period. But maize apparently was not introduced into the East any earlier than about A.D. 200-300 (Ford 1987, Chapman and Crites 1987, Yarnell and Black 1985:102; Smith 1985a:51), and food production systems dominated by maize did not develop in the eastern woodlands of North America until after A.D. 1000 (Smith 1985a:51, 1986).

When it first arrived in the eastern woodlands maize was adopted as just another starchy seed crop within already established "pre-maize" plant husbandry systems which centered on a group of six plant species: sumpweed *(Iva annua),* sunflower *(Helianthus annuus),* goosefoot *(Chenopodium berlandieri),* [1] erect knotweed *(Polygonum erectum),* maygrass *(Phalaris caroliniana),* and little barley *(Hordeum pusillum)* (Yarnell and Black 1985). The two pre-maize oily-seed crops, sunflower and sumpweed, were long-standing domesticates in the East, as was at least one of the starchy-seed crops—a thin testa cultivar chenopod (Smith 1987a, Smith and Cowan 1987). When maize was initially introduced into the East at ca. A.D. 200-300, these three long standing domesticates had recently been joined by a no-testa or naked cultivar chenopod (Fritz 1986), as well as by, perhaps, a domesticated variety of knotweed (Fritz 1987). Little barley and maygrass, both spring harvested starchy-seed crops, were also important in pre-maize food production economies, even though no archaeological specimens of either species have *as yet* been demonstrated to exhibit morphological changes associated with domestication.

Variation in the relative abundance of seeds of these pre-maize cultigens in archaeobotanical assemblages from different regions of the East suggest both geographical and temporal diversity in their economic importance (Smith 1985a:52). These assemblages

also reflect a fairly uniform increase in the importance of such regionally diverse pre-maize plant husbandry systems across the mid-latitude interriverine area of the East at about 2,500-2,000 B.P. (Smith 1985a:52,70), and these indigenous seed crops appear to have continued in importance up through the post A.D. 1000 transition to maize agriculture. But they seem to have all but disappeared from view by the time of European contact. With the possible exception of Harriot's 1586 account from the Carolinas of sunflower and ***melden*** (Sturtevant 1965), and LePage's eighteenth century description of ***choupichoul*** being grown by the Natchez (Smith 1987b), the ethnohistorical record is silent in regard to possible remnant pre-maize cultigens of the eastern woodlands.

In the past decade the developmental trajectory of these pre-maize plant husbandry systems, from initial emergence to eventual decline, has attracted increasing research interest because of their likely central role in the evolution of more complex Woodland period (2500-1200 B.P.) prehistoric societies in the prehistoric East. One important aspect of gaining a better understanding of these early cultigens involves attempting to determine their economic potential through both nutritional analyses of seeds, and present day harvesting experiments to establish their potential yield. The nutritional composition of seeds has been established for *Polygonum erectum* (Asch and Asch 1985:361); *Chenopodium berlandieri/bushianum* (Asch and Asch 1985:361); *Phalaris caroliniana* (Crites and Terry 1984); *Iva annua* (Asch and Asch 1978); and *Helianthus annuus* (Earle and Jones 1962, Watt and Merrill 1963; Jones and Earle 1966:15). In addition, Seeman and Wilson (1984) and Murry and Sheehan (1984) provide nutritional composition information for other species of *Polygonum* and *Chenopodium*. Efforts to obtain potential harvest yield information for these six pre-maize crops, on the other hand, have proven difficult because of the apparent present-day absence of large wild or weedy stands suitable for harvest studies. Because of this difficulty modern harvest yield research has yet to be carried out on either maygrass or little barley. Potential harvest yield estimates for sunflower are derived from modern commercial field crop statistics (e.g. Martin and Leonard 1967:932-933). In addition, potential yield information for *Polygonum erectum* is limited to a single 25 ft^2 (2.29 m^2) stand harvested by Murray and Sheehan during their study of five species of *Polygonum* in Illinois and Indiana (1984:288, 290-291). Similarly, harvest yield projections for *Chenopodium berlandieri/bushianum* are based on two isolated plants, each occupying about 1 m^2 (Asch and Asch 1978:313). Unable to locate any populations of *Chenopodium berlandieri* large enough to sample repeatedly, Seeman and Wilson (1984) provide detailed harvest yield information for *Chenopodium missouriense* stands harvested in Indiana. *Iva annua* is the best documented of these six early cultigens in terms of both harvest rate values (based on 20 timed collections from seven stands) and harvest yield values (based on eight total yield collections from 1 m^2 plots) (Asch and Asch 1978).

In light of their central role in fueling prehistoric Woodland period cultural change, it is surprising that information concerning the potential prehistoric economic potential of these pre-maize seed plants is so limited, and that so few present-day wild stands have been harvested. In order to begin to fill this gap in our understanding of these prehistoric crop plants, and to learn more about their habitat requirements, present day geographical ranges, and harvest yield potential, I began, in the fall of 1984, an annual fall harvesting circuit through the eastern United States. For the first two years field studies focused on *Iva annua* and *Chenopodium berlandieri*, and this article reports the results of the 1984 and 1985 harvests in regard to *Chenopodium berlandieri*.

METHODS

One of the main goals of the 1984-1985 fall harvesting project was to locate and harvest stands of *Iva annua* and *C. berlandieri* over a broad area of the Midwest and

Southeast. Accordingly, instead of spending an extended period of time in any one area, I selectively concentrated my survey efforts in those high probability target habitats I thought likely to support stands of either of these two species. Because sumpweed was known to be a component of early successional floodplain communities, I selected a route of travel through the East that crossed and recrossed a number of river valleys, and surveyed at each river crossing, up and down the floodplain for several miles in search of stands of *Iva annua* and *C. berlandieri*.

These two species frequently colonize recently disturbed soil in both floodplain and upland locations, and are often found growing on construction sites, in vegetable gardens and agricultural fields, as well as in a wide range of other settings, including vacant lots, abandoned gas stations, playgrounds, parking lots, cotton gin waste dumps, dredging spoil piles, under bridges, etc. Such disturbed ground habitat settings were easy to recognize from a moving car, and I frequently stopped to investigate them, particularly when chenopod plants were observed in passing. Although most chenopod stands occurring in such disturbed soil situations were dominated by the ubiquitous *C. missouriense*, pockets of *C. berlandieri* were occasionally located within larger stands of *C. missouriense*. By combining the deliberate surveys of river valley segments with more opportuistic surveys of upland disturbed soil situations as they were observed, I was able to investigate a large number of chenopod stands while still traveling over 200 miles per day.

Each *C. berlandieri* stand located was assigned a field catalog number and this, along with its geographical location and habitat setting and plant characteristics, were recorded in a field notebook. Stand and individual plant characteristics which were described included: (1) Stand size in square meters; (2) Individual plant height in centimeters; (3) Plant habit and habitat—the shape and habitat setting of plants (e.g. slender and unbranched, growing in dense cover, light shade); (4) The color and condition of individual plants.

Harvesting was accomplished by hand stripping individual infructescences (fruit clusters) into a bag attached to the belt loops of the collector. Large plastic garbage bags served admirably in this capacity when simply twisted through belt loops and tied off, leaving both hands free for me to grasp an infructescence in a closed fist and strip it from the plant into the waist bag (see Seeman and Wilson 1984:305 for other harvesting options for *Chenopodium)*. Harvest times for individual plants were recorded in most cases. Each evening, after removing the infructescence material harvested that day from the plastic waist bags, I placed it on newspaper overnight to partially dry, then repackaged it in paper bags for further drying. When I found a stand containing *C. berlandieri* plants which still retained leaves, a voucher specimen was pressed for the National Herbarium (NMNH). Occasionally I also collected entire plants after stripping their infructescences, returning them to the lab to be dried and weighed.

Subsequent to thorough drying and weighing in the laboratory, harvested fruit was winnowed from perianth, leaf and stem fragments[2]. Cleaned fruits comprised from 65% to 79% of unwinnowed material (Table 1).

TABLE 1.—*Harvest Yield Information for 86 Chenopodium berlandieri Plants.*

Catalog Number	Plant Height (cm)	Plant Weight (gms)	Unwinnowed Fruit Wt. (gms)	Winnowed Fruit Wt. (gms)	Unwinnowed /Winnowed Ratio	Harvest Time (min.)
20	122	8.6		4.62		
21	127	18.2		27.19		
22	84	7.7		5.31		

TABLE 1.—*Harvest Yield Information for 86 Chenopodium berlandieri Plants.* (continued)

Catalog Number	Plant Height (cm)	Plant Weight (gms)	Unwinnowed Fruit Wt. (gms)	Winnowed Fruit Wt. (gms)	Unwinnowed /Winnowed Ratio	Harvest Time (min.)
23	112	13.1		12.88		
24	79	4.3		3.66		
25	46	4.7		3.73		
26	56	5.1		4.01		
27	61			14.76		
28	183			72.4		
39	152			109.0		7:00
40	124			11.14		0:45
41	58			2.98		0:15
42	142			19.17		1:00
82A	183	160.0		49.0		2:00
87	200	126		91.8		8:00
89A	58	16.8		12.6		1:00
89B	86	36.4		38.4		2:00
90A	105	220.0	75.7	55.2	73	2:00
90B	83	95.3	41.9	30.3	72	1:30
90C-1		5.4	3.3	2.3	73	0:15
90C-2		17.3	11.0	8.4	77	0:15
90C-3		24.1	22.6	17.3	77	
90C-4	48	1.4	.9	.7	78	
90C-5		10.5	4.0	2.8	70	
91	150	497.5		428.1		16:00
92A		3.2		2.6		
92B		18.5	11.7	8.5		
92C		18.5		16.7		
92D		12.5		8.6		
92E	21.1		18.6			
92F		10.7	5.6	3.9		
92G		26.1		31.5		
92H		23.1		20.5		
92I		60.0	30.4	19.7	65	
92J		82.2	103.8	71.6	69	
94A	125-150		93.3	71.7	77	8:00
94B	125-150		148.2	101.6	68	8:00
94C	125-150		144.7	100.8	70	11:00
94D	175-200			127.3		10:00
94E	125-150			89.1		6:00
94F	100-125			74.4		9:00
94G	100-125			10.8		1:00
94H	175-200		71.5	56.7	79	4:40
94I	150-175			9.7		1:40
94J	75-100			5.1		1:00

TABLE 1.—*Harvest Yield Information for 86 Chenopodium berlandieri Plants.* (continued)

Catalog Number	Plant Height (cm)	Plant Weight (gms)	Unwinnowed Fruit Wt. (gms)	Winnowed Fruit Wt. (gms)	Unwinnowed /Winnowed Ratio	Harvest Time (min.)
94K	75-100			20.6		1:00
94L	75-100			14.9		2:12
94M	100-125			58.5		2:30
94N	75-100			10.4		0:49
94O	75-100		21.4	16.6	78	1:19
94P	100-125			41.4		3:15
94Q	125-150			61.5		3:55
94R	125-150			76.0		6:30
94S	100-125			73.0		4:00
94T	100-125			33.3		1:30
94U	50- 75			7.0		0:54
94V	100-125			34.0		5:15
94W	125-150			68.0		7:00
94X	100-125			79.2		5:00
94Y	75-100			32.2		1:30
94Z	175-200			84.6		4:17
94AA	75-100			36.2		4:00
94BB	100-125			80.5		4:30
94CC	175-200		230.6	175.5	76	11:00
94DD	100-125			32.2		2:00
94EE	75-100			15.0		2:27
94FF	75-100			9.1		0:15
94GG	75-100			1.5		0:30
94HH	75-100			9.5		1:13
94II	100-125			78.7		6:00
94JJ	125-150			66.8		5:10
95A	75-100			35.0		4:05
95B	75-100			26.8		2:13
95C	75-100			31.0		3:40
95D	125-150			129.8		10:10
95E	75-100			23.4		1:27
95F	75-100			13.4		0:30
95G	75-100			22.3		2:45
95H	175-200			30.5		3:34
95I	75-100			5.0		0:45
95J	125-150		83.2	54.4	65	4:30
96	100			49.2		3:00
101A	200	32.2		9.1		
101B	100			0.3		
105	86	5.0	2.6	2.0	77	
108	60		4.5	3.4	76	

RESULTS

During harvesting trips through the eastern United States in the autumn of 1984 and 1985, *C. berlandieri* stands were harvested in Maryland, Pennsylvania, Ohio, Michigan, Missouri, Tennessee, Arkansas, Alabama, and South Carolina. I recorded individual plant yield information for a total of 86 *C. berlandieri* plants (Table 1), and carried out timed harvest experiments on 16 *C. berlandieri* stands (Table 2).

TABLE 2.—*Harvest Yield Information for Sixteen Stands of Chenopodium berlandieri.*

Catalog Number	Stand Area (square meters)	Number of Plants	Fruit Weight (grams)	Harvest Yield (kg/ha)	Harvest Time (min)	Harvest Rate (kg/hour)
30	250	4000(?)	459.8	—	22	1.250
39-42	1	4	142.3	1423	9	.949
57	1	10	108.9	1089	9	.726
58	1	9	83.3	833	6	.832
59	1	7	121.3	1213	8:30	.856
82A	1	3	49.0	490	2	1.470
87	1	1	91.8	918	8	.689
88	2	58	112.6	563	5	1.350
890	1	2	51.0	510	3	1.020
90	1	4	95.9	959	4	1.438
91	1.5	1	428.1	2854	16	1.605
92	3	10	202.2	679	12	1.010
94 Top Ten Plants						
94CC	1	1	175.0	1750	11	.955
94D	1	1	127.3	1273	10	.764
94B	1	1	101.6	1016	8	.762
94C	1	1	100.8	1008	11	.540
94E	1	1	89.1	891	5	1.060
94Z	1	1	84.6	846	4:17	1.190
94BB	1	1	80.5	805	4:30	1.073
94X	1	1	79.2	792	5	.950
94II	1	1	78.7	787	6	.786
94R	1	1	76.0	760	6:30	.702
94 Top Ten Plants (clustered)(a)	10	10	1064.0	1064	71	.899
94 All 36 plants (clustered)	32	36	1860	580	148	.754
95D	1	1	129.8	1298	10	.778
95 All Ten Plants (clustered)	10	10	371.6	371	34	.655
96	1	1	49.2	492	3	.984
97	3.5	27	96.7	276	14	.414

(a) The individual plants comprising stands 94 and 95 were fairly widely scattered across a heavily overgrown soybean field (Figure 5). In order to estimate what the harvest yield values for each of the stands would be if the plants were not so scattered, "clustered" statistics were obtained by centering each plant in an arbitrary 1 meter square and then assuming that the one meter squares adjoined each other.

Because of the elusive nature of present-day stands of *C. berlandieri*, the absence of previous research, and the need for additional harvest experiments on modern descendant populations of this important prehistoric cultigen, I provide the following brief, if admittedly tedious, descriptions of the stands harvested in 1984 and 1985.

Wayne County, Michigan (Catalog numbers 20-30, 87-88).—Catalog numbers 20-28 were assigned to individual *C. berlandieri* plants harvested in October 1984 from a disturbed soil setting in a vacant lot in Grosse Pointe, Michigan. Large, often dense (>50 plants per m^2), stands of both *C. berlandieri* and *C. missouriense*, frequently mixed, occurred in both full sun and light shade. A mixed stand of *C. berlandieri* and *C. missouriense* covering an area of approximately 250 m^2 and located in partial shade was assigned field number 30 and selectively harvested in October, 1984. With an approximate average density of 50 plants per m^2 (4 plots, counted, each 1 m^2), the stand contained about 12,000 plants, ranging in height from 30-130 cm, with few lateral branches. As is almost always the case in upland mixed stands, *C. missouriense* was the more abundant of the two species, comprising over three-fourths of the stand. Rather than being scattered randomly, *C. berlandieri* plants formed a number of discrete pockets within the larger stand. The largely leafless, distinctively mustard colored *C. berlandieri* plants had dark, large fruited, and glomerate infructescenses. I could easily distinguish them, both by touch and sight, from the still leafed purple stemmed *C. missouriense* plants had dark, large fruited, harvesting of the stand relatively easy. Moving through the stand and selecting the largest and most visible infructescences, I was able to hand strip 300 *C. berlandieri* infructescences into a waist bag in 22 minutes, yielding 575 g of infructescence material and 460 g of clean fruit after winnowing.

Returning to this vacant lot in October of the following year I harvested an isolated, still green, 2 m high *C. berlandieri* plant growing in full sun, and having 28 lateral branches (Catalog number 87). In addition, I harvested a 2m^2 cluster of 58 *C. berlandieri* plants (Catalog number 88, Table 2) found growing within a 15x20 m stand of *C. missouriense*.

Fulton County, Pennsylvania (Catalog numbers 38-42).—Catalog numbers 38 (herbarium voucher) and 39-42 were assigned to 5 individual plants harvested from a 1m^2 cluster of *C. berlandieri* found within a surrounding 4m^2 stand of *C. missouriense* growing in a construction site dirt pile.

Mississippi County, Arkansas (Catalog numbers 57-59).—A 5x7 m stand of *C. missouriense* was observed growing in full sun on a roadside dirt pile in November of 1984. Catalog numbers 57, 58 and 59 were assigned to three separate clusters of *C. berlandieri* located within the larger dense stand of *C. missouriense*. Each less than 1 m^2 in area, these clusters contained 10, 9 and 7 plants respectively. The *C. berlandieri* plants ranged in height from 130-150 cm, had few lateral branches within a meter of the ground, and could be visually distinguished from the surrounding, still green, *C. missouriense* plants by their grayish brown leafless condition and generally smaller size.

Cherokee County, South Carolina (Catalog numbers 82A-82Q).— Three clusters of *C. berlandieri* were observed growing adjacent to a bridge over the Broad river in November, 1984. Two of the clusters (82 A-C, 82 D-G) were approximately 1 m^2 in area, while the third (82 H-Q) was 1x3 m in size. All of the plants were dead and had lost the majority of their fruit.

Prince Georges County, Maryland (Catalog numbers 89A, 89B).—In the spring of 1985 *C. berlandieri* seeds were scattered in turned soil adjacent to garden plots at the Museum

Support Center, Smithsonian Institution, in Silver Hill Maryland. Only two plants (89A, 89B) grew to maturity, however, likely due to a very dry growing season and a failure to expose the seeds to freezing temperatures during the preceding winter. Upon harvesting them in October, both plants were dry, brown, with partial seed loss, and were choked by a dense growth of grass and weeds.

Washington County, Maryland (Catalog number 90A, 90B, 90C1-90C5).—In November, 1985, I observed *C. missouriense* growing in fill dirt along a roadedge guardrail. Within this stand I harvested a 1x2 m cluster of *C. berlandieri* containing two large (90A, 90B) and five smaller (90C1-90C5) plants. These seven plants were all dead, brown, and dry, with partial seed loss, and could be easily distinguished from the surrounding, still green *C. missouriense* plants. The two larger plants, along with two of the smaller plants (90C-1, 90C-2) occupied an area of less than 1 m^2.

Pike County, Ohio (Catalog numbers 91, 92A-92J).—Catalog number 91 was assigned in November 1985 to a single *C. berlandieri* plant growing in an overgrown creek bottom vegetable garden (corn, pumpkins, sunflowers, summer squash) (Fig. 1). Occupying a 1x1.5 m area, the large (height 1.5 m), bright yellow leafless plant had numerous lateral branches with black terminal infructescences (Fig. 2). Little seed loss was apparent. After hand stripping, the plant was felled and subsequently weighed in the laboratory. Occupying a 1x3 m area, a stand of ten *C. berlandieri* plants (92A-92J) was found growing in dense undergrowth in the same garden (Fig. 3). The plants all had leafless yellow stalks and black infructescences, and could be quickly distinguished from the surrounding dense undergrowth.

Mississippi County, Missouri (Catalog numbers 94A-94JJ, 95A-95J, 96, 97).—Forty six *C. berlandieri* plants (94A-94JJ, 95A-J) were found growing in an overgrown soy bean field

FIG. 1.—Overgrown creek bottom vegetable garden location of *Chenopodium* stands 91 and 92. Pike County, Ohio.

FIG. 2.—A large *Chenopodium berlandieri* plant, field catalog number 91, which yielded 428 gm of seeds (surrounding weeds removed prior to photograph).

in November, 1985. Separated from the main channel and sand bars of the Mississippi River on the east by a 20-30 m wide black willow vegetation zone, and from the levee on the west by a 100 m wide lowlying area of wet clay soil (Fig. 4), the soybean field was about 70 m wide and paralleled the river for approximately 160 m. The mature but unharvested field of soybeans, which appeared not to have been sprayed with herbicides and to have been generally neglected, was thickly overgrown with weeds, including johnson grass, wild bean *(Strophostyles helvola)* and both *C. missouriense* and *C. berlandieri*. In contrast to previously observed upland situations where these two chenopod species were found growing in the same locales, *C. missouriense* was not a dominant constituent of the weed plant community (see Seeman and Wilson 1984:305). *C. berlandieri*, on the other hand, was quite abundant in the field.

C. Wesley Cowan and I mapped, described, and harvested a rather scattered linear stand of 46 plants over a period of 4 hours (Fig. 5). After being mapped and assigned a letter code, the height of each plant was recorded, along with information regarding stalk and infructescence color and condition, the relative abundance of lateral branches, and the presence/absence of wild bean vines. Catalog number 96 was assigned to a single *C. berlandieri* plant located 20 m from the main channel of the Mississippi River. Growing in full sun on the sand beach of the river, and partially entangled by *Strophostyles helvola*, this specimen was brown and dry, with numerous lateral branches (Fig. 6). After locating catalog specimen 96 along the sandy beach edge of the Mississippi River, C. Wesley Cowan and I walked further south along the river for a distance of about 400 m within the relatively narrow (20-50m) black willow vegetation zone which paralleled the river's edge. In the light shade setting under a willow canopy, *C. berlandieri* plants were quite common, often occurring in linear stands paralleling the river. In contrast to the habit of *C. berlandieri* plants observed growing in full sun, these partial shade understory plants

FIG. 3.—The dense undergrowth setting of *Chenopodium berlandieri* stand 92.

had thin straight stalks and lateral branches having few leaves and small diffuse infructescences (Fig. 7). In addition, these understory plants were still green and in full leaf. We harvested a stand of 27 plants occupying a 3.5 m^2 area (Fig. 7).

Hardin County, Tennessee (Catalog numbers 101A, 101B).—Two *C. berlandieri* plants were found growing in a bank slump area along the Tennessee River and harvested in November of 1985. Located in partial shade, both plants were similar in habit to the understory plants described above (97), having tall thin stalks and lateral branches with diffuse infructescences. Both plants were leafless and brown, with 101A exhibiting little apparent fruit loss, while 101B had lost most of its fruits.

Cullman County, Alabama (Catalog number 105).—In November, 1985 I located a stand of 4 *C. berlandieri* plants in a flat floodplain area only 5 m from the edge of the Black Warrior River. All four plants were still green and were in excellent condition for herbarium specimens. Three of the four were pressed for the collections of the National Herbarium (NMNH), while the fourth was hand stripped for the present study.

Tuscaloosa County, Alabama (Catalog number 108).—In November 1985 I located a single plant in thick undergrowth at the top of a steep sand bank down to the Black Warrior River. Growing in full sun, the plant was still green but leafless when harvested.

FIG. 4.—Looking east from the Mississippi River levee toward the soybean field location of *Chenopodium berlandieri* stands 94 and 95. The main channel and opposite shore of the Mississippi River are visable in the background, along with exposed sand bars. The low clay soil area separating the levee from the soybean field is visible in the foreground.

DISCUSSION

The Habitat of C. Berlandieri In The Eastern Woodlands.—The field research phase of this study confirmed what a number of previous researchers had noted—*C. berlandieri* is an elusive subject for harvest yield experiments since it is not generally abundant nor does it frequently occur in large stands in the Eastern United States (Seeman and Wilson 1984:303,304; Asch and Asch 1977:25; Munson 1984). As a result of the field research reported here, the preferred habitat situation of this species can now be described with a greater degree of accuracy, which should facilitate the location of stands for future harvest yield studies.

The primary habitat of *C. berlandieri* in the Eastern United States is river valley floodplains, particularly large meandering rivers such as the Mississippi River and its major tributaries. Within this river valley alluvium habitat zone *C. berlandieri* can be found growing in a number of different disturbed soil situations. Primary among these is as an abundant understory constituent of black willow river margin sand bank vegetation communities. These "naturally disturbed" river margin black willow zones are subject to annual scouring by floodwaters and deposition of alluvial, primary heavy fraction sandy, soil. Within this shady understory setting *C. berlandieri* plants are tall, slender, have small diffuse infructescences, and a quite low seed yield per square meter value (Table 2, Fig. 8, catalog number 97). Wahl (1954:44) briefly mentions this river margin understory habitat setting for *Chenopodium berlandieri: "C. bushianum* occurs most often as a weed of cultivated places but is found also in alluvium along streams and in waste places. The shade form is more delicate." Steyermark (1963:614) also mentions this habitat: *C. bushianum* "occurs in sandy fields and alluvial ground along rivers, waste

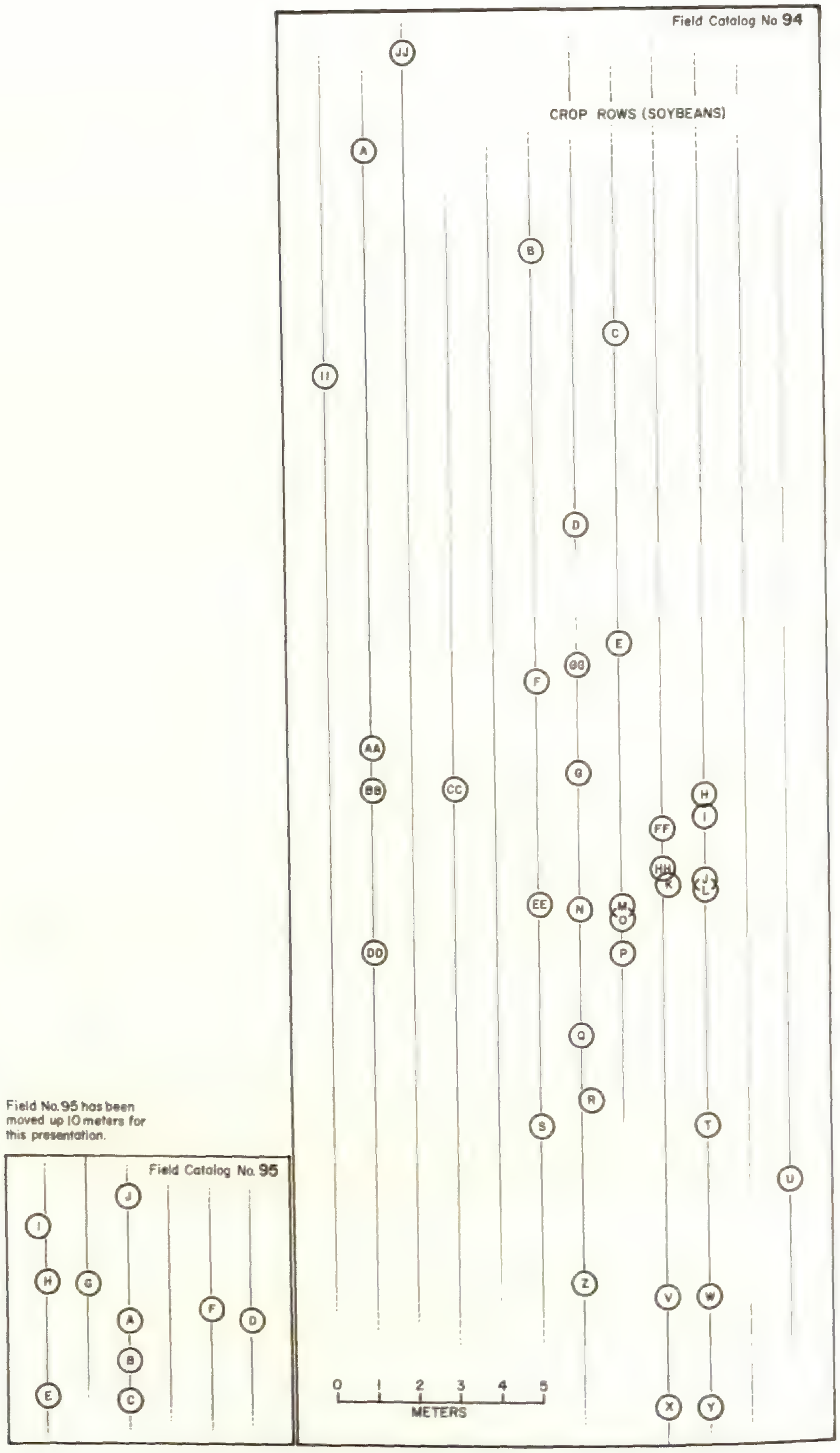

FIG. 5.—The location of the 46 *Chenopodium berlandieri* plants comprising stands 94 and 95. Illustration by Britt Griswold.

FIG. 6.—A *Chenopodium berlandieri* plant growing on the sandy bank of the Mississippi River.

places, wooded slopes, and dry open or shaded ground." In addition, a Field Museum of Natural History herbarimum specimen identified as *C. bushianum* and examined by Asch and Asch (1977:36) carried the following habitat description: "riverbank among trees—Fargo, North Dakota."

While this understory setting can be considered the primary "natural" floodplain habitat of *C. berlandieri*, the plant also occurs in two other nearby "naturally disturbed" floodplain situations. Jackson's statement (1985:183) that "stands of chenopods also can be found along bank margins left bare by receding summer water level" corresponds with field observations made in November 1985 regarding *C. berlandieri*. As the water level of the Mississippi River receded during the summer and fall, the exposed river margin sand beach supports multibranched *C. berlandieri* plants with large terminal infructescences (Table 1, Fig. 6, catalog number 96). Unstable and actively eroding river and terrace banks and bluffs also support occasional, often solitary, *C. berlandieri* plants, particularly along the upper bank edge (Table 1, catalog numbers 101, 105, 108; Asch and Asch 1977:20; Munson 1984:381, 383-384). It is important to note that while *C. berlandieri* can be recognized as occurring in at least three different "naturally disturbed" habitat settings within river valley floodplains, Styermark's (1963:614) reference (quoted above) to the plant's association with sandy soil is quite perceptive in that *C. berlandieri* rarely occurs in other floodplain soils of heavier texture. Although Asch and Asch (1977) describe a late season stand of *Chenopodium berlandieri* growing on an exposed mudflat in the lower Illinois River Valley, such stands are rare. During

FIG. 7.—The light shade black willow understory setting of *Chenopodium berlandieri* stand 97.

extensive surveys by Klein, Daley and Wedum (1975) and by Munson (1984) of Illinois and Mississippi River Valley mudflats exposed during seasonal drying of shallow lakes, ponds, and sloughs no chenopod species were noticed in mudflat vegetation communities.

In addition to occuring in these three overlapping naturally disturbed sandy soil floodplain situations, *C. berlandieri* can also be found in a variety of anthropogenically disturbed soil settings. It is often the dominant chenopod weed in river valley fields and gardens (catalog numbers 91, 92, 94, 95). Wilson and Walters (n.d.) describe the habitat of *C. bushianum* as "disturbed ground, especially alluvial soil of agricultural areas", while Styermark (1963:614) has it occurring" . . . in sandy fields and alluvial ground along rivers", and Wahl (1954:44) considers it as "A weed of cultivated places . . . ", also found "in alluvium along streams and in waste places." While floodplain sandy soil fields and

gardens can be considered the primary "anthropogenic" disturbed habitat setting for *C. berlandieri* in the Eastern United States, it is also found in other disturbed soil and waste place settings in both river floodplains (under bridges— catalog number 82) and upland areas (fields, gardens, construction sites and along highways). *C. berlandieri* occurs only infrequently in upland settings where it is rarely alone, but is almost invariably found growing in small scattered pockets, as a minor constituent within larger stands dominated by *C. missouriense* (catalog numbers 20-28, 38-42, 57-59, 87-88, 90) (Asch and Asch 1977:25-26; Seeman and Wilson 1984:303-305).

In summary, *C. berlandieri* in the Eastern United States is an early successional plant species inhabiting a variety of different natural and anthropogenically disturbed sandy soil situations within river valley floodplains. Within such river valley floodplains environments it would not be at all unusual to find *C. berlandieri* inhabiting a number of adjacent habitats all situated within 50 m of each other (exposed low water river margin sand beaches, black willow understory, eroding terrace sand banks, and floodplain agricultural fields). It is also an infrequent minor constituent in upland disturbed soil area chenopod stands. It is thus within the meander belt of the Mississippi River and its tributaries, adjacent to active main channels, that stands of *C. berlandieri* suitable for harvest studies are most likely to be encountered. Within this area, poorly weeded and overgrown natural levee fields and gardens provide the best opportunity for locating extensive stands of multiple branched plants. That floodplain gardens and fields hold the best promise for future harvest yield studies of this plant is underscored by the results of the 1984-1985 fall harvest project. While only four of the 16 stands harvested were situated in floodplain fields, those four stands (91, 92, 94, 95) yielded two-thirds of the total seed collected. Fortunately, such present-day floodplain fields and gardens also represent essentially the same setting within which prehistoric cultivation of *C. berlandieri* would have primarily occurred.

The Economic Potential Of C. Berlandieri.—Yield values obtained for the 16 *C. berlandieri* stands harvested in 1984 and 1985 show considerable variation (Table 2, Fig. 8), from a low of 276 kg/ha recorded for field catalog number 97—the 3.5 square meter black willow, partial shade stand, to a high of 2854 kg/ha for stand 91, a single plant occupying 1.5 m^2 and yielding 428 g of seed. This variability is to be expected because of considerable variation in both the amount of sun received by different stands and the degree of crowding and competition from surrounding plants. Variation in plant height to seed yield ratios and plant weight to seed yield ratios (Table 1) also reflect differing degrees of competition and sunlight. Substantial seed loss prior to harvesting was only occasionally a factor in harvest yield variation (82A, 101B). The long period seed retention of *C. berlandieri* is well documented (Wahl 1954, Seeman and Wilson 1984:303, 309). The application of fertilizers and herbicides played undocumented roles in influencing yield values.

In the only previous published consideration of the economic potential of *C. berlandieri*, Asch and Asch (1978:313), present yield values of 1330 and 1740 kg/ha for two tall multiple branched plants, each growing in an open full sun plot "in which the plants had maximum potential for vegetative growth." Because of the limited competition represented in the case of these two plants, Asch and Asch suggested that it was "probably unreasonable to extrapolate its production to a large stand where the chenopods would be competing with each other" (1978:313). While the harvest yield values presented by Asch and Asch fall toward the upper end of the range of yield values recorded in the present study (Fig. 8), I think that they do not represent unreasonably high approximations for prehistoric *Chenopodium* production in the Eastern Woodlands. Their average yield value (1535 kg/ha) is about half that of the most productive (plant) stand in the present study (field catalog number 91, yielding 2854 kg/ha), and falls about midway along the documented range of variation (276 - 2854 kg/ha). In addition, with the exception

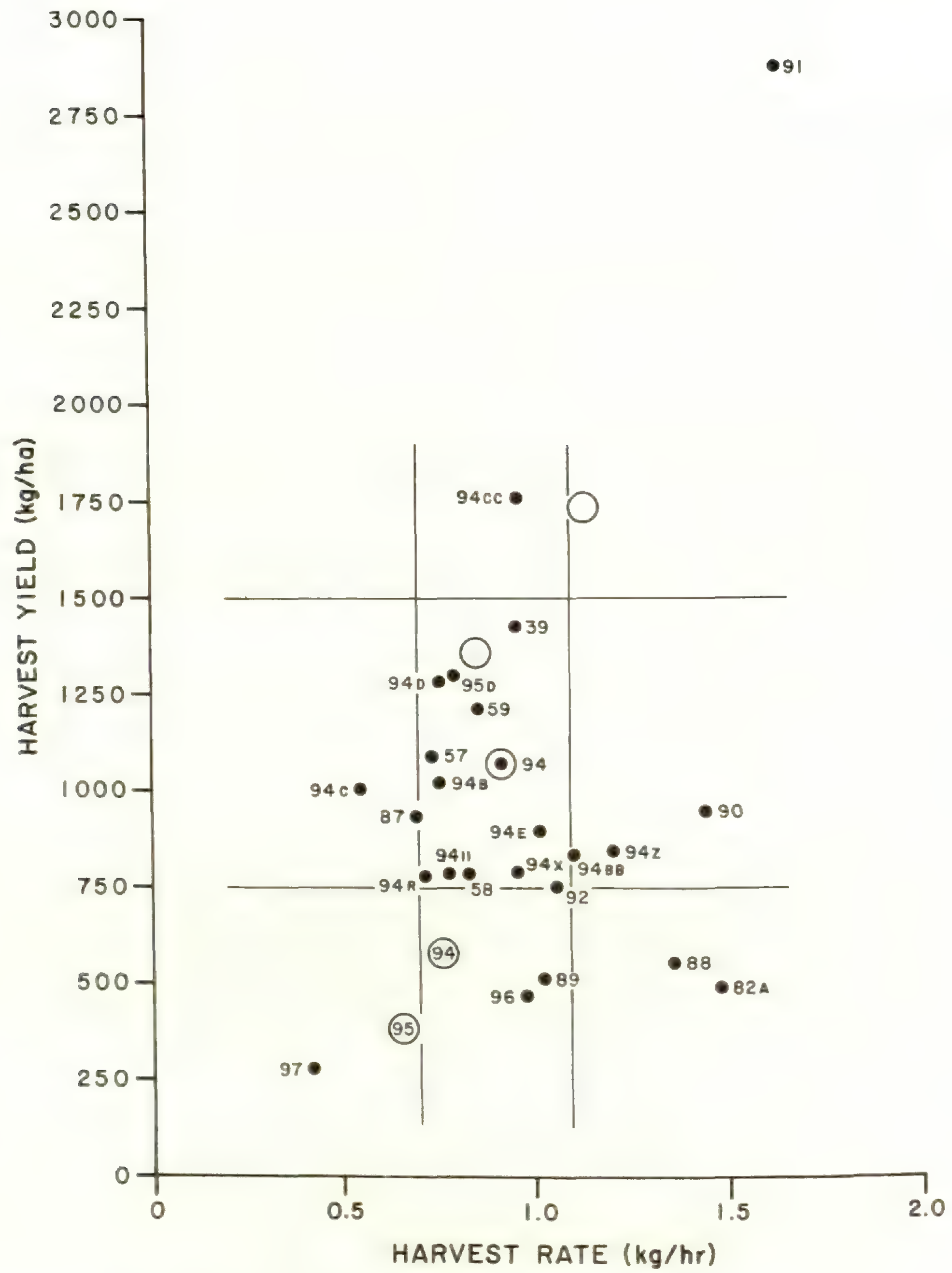

FIG. 8.—Harvest yield (kilograms/hectare) and harvest rate (kilograms/hour) values for 16 *Chenopodium berlandieri* stands harvested in 1984 and 1985, compared with the single plant yields reported by Asch and Asch (1978). See Table 2 for explanation of "clustered" values. Range estimates for harvest yield values (750-1500 kg/ha) and harvest rate (.7 - 1.1 kg/ha) are indicated.

of stands 82a and 87, all of the plants harvested in the present study were competing with other plants, either as small pockets surrounded by larger stands of *C. missouriense*, or in dense undergrowth situations (Figs. 1, 3, 9). As a result, with the exception of stand 91, the yield values obtained from the 16 stands harvested in 1984 and 1985, as presented in Fig. 8, could be considered as defining the lower end of the range of likely prehistoric

FIG. 9.—The dense weedy growth setting of *Chenopodium berlandieri* stand 94. An exposed sand bar of the main channel of the Mississippi River is visible in the background.

harvest yield levels for *C. berlandieri*—what might be expected from poorly maintained and overgrown fields. A relatively conservative range estimate of 750-1500 kg/ha for prehistoric yield levels for *C. berlandieri* is proposed (Fig. 8).

Harvest rate values obtained during this study also showed considerable variation, with stands 97 and 91 again providing minimum (.41 kg/ha) and maximum (1.6 kg/hr) values, respectively. Variability in harvest rate values was due primarily to differences in the size and degree of compactness, and terminalization of infructescences of harvested plants. The light shade plants of stand 97 had numerous small and diffuse infructescences which took far longer to locate and to strip than the large compact fruit clusters of stands 91-92 and 94-95 (Fig. 10). Surrounding chenopod plants and weeds, particularly wild bean *(Strophostyles helvola)* also substantially decreased harvest rates by making it more difficult to see and reach infructescences. A relatively conservative harvest rate range estimate of 0.7-1.1 kg/hr. is proposed for *Chenopodium berlandieri* in the prehistoric Eastern Woodlands (Fig. 8).

Harvest Yield Comparisons.—In order to make any harvest yield comparisons between *C. berlandieri* and other crop plants of the prehistoric eastern woodlands of North America, it is first necessary to take into consideration the thick seed coat of the modern wild/weedy populations of *C. berlandieri* harvested in the present study. Ranging in thickness from 40-70 microns (Smith 1985b), the seed coat of wild/weedy fruits consists largely of non-nutritive fiber, and accounts for at least 30% of the seed weight (Seeman and Wilson 1984).

Of the two prehistoric domesticated varieties of *C. berlandieri* cultivated in the prehistoric eastern woodlands, one, *C. berlandieri* ssp. *jonesianum*, had a thin (20 microns) testa (Smith and Funk 1985), and the other lacked a testa entirely (Wilson 1981, Fritz 1986). Harvest yield rates, at least 30% lower could therefore be expected for prehistoric cultivated stands producing the same number of fruits per hectare as modern wild/weedy stands. Reducing harvest yield values obtained during the present study by 30% (from 750-1500 kg/ha to 525-1050 kg/ha) results in a closer approximation of the nutritive yield of thin testa and testa-less prehistoric cultigen varieties (Fig. 11).

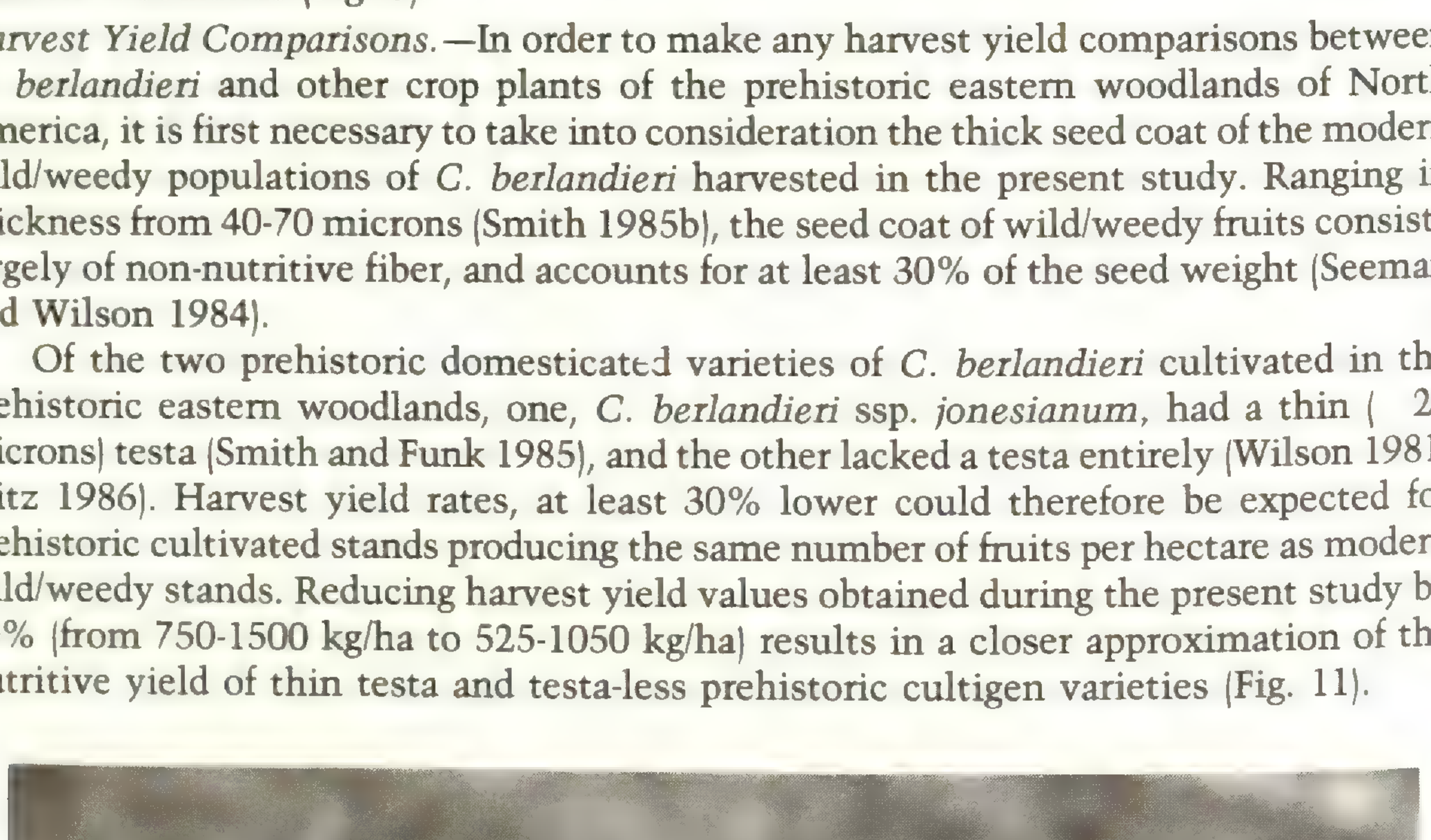

FIG. 10.—The typical infructescence form of *Chenopodium berlandieri* plants growing in full sun.

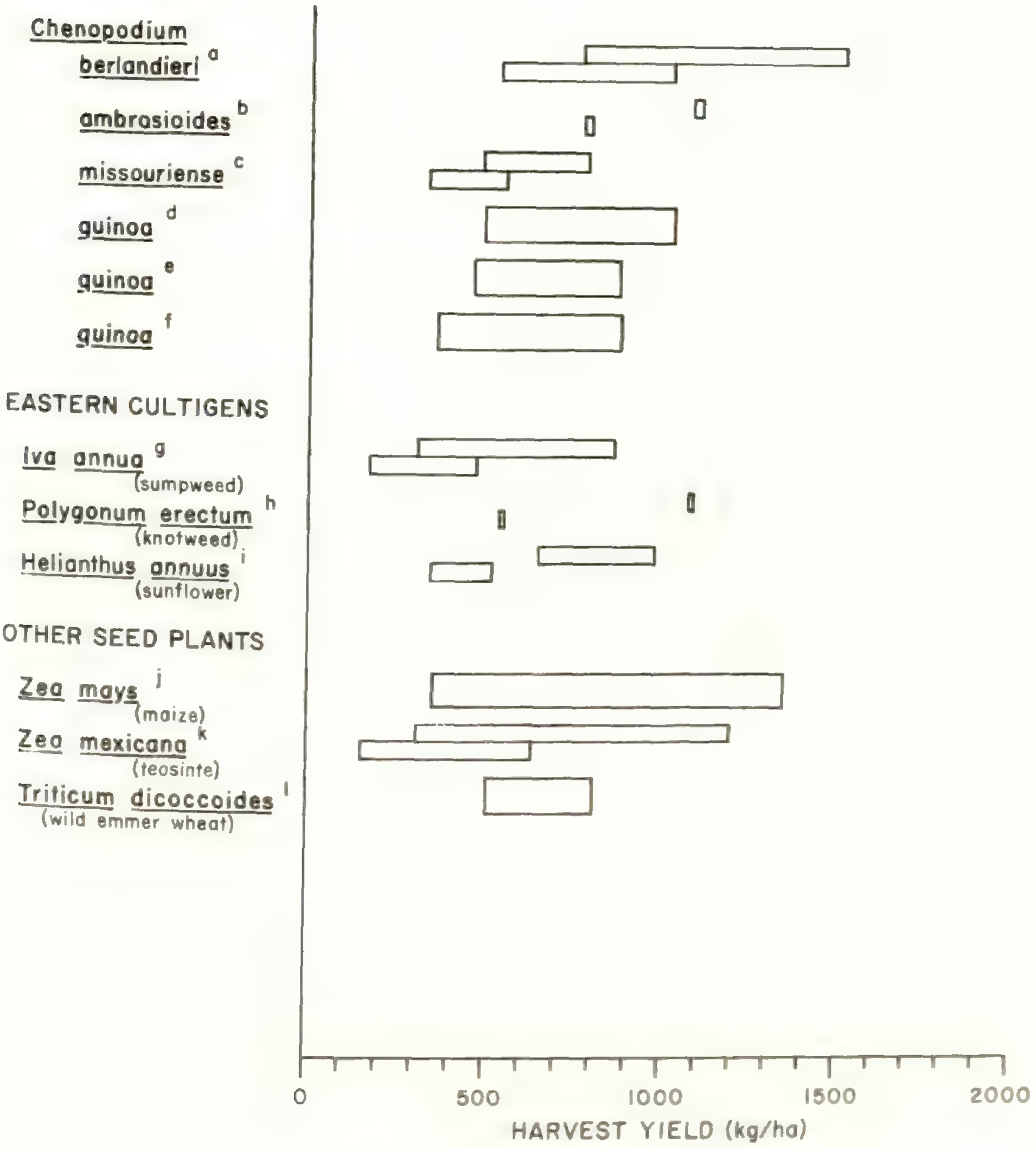

FIG. 11.—Range estimate harvest yield values for ten seed plants. To facilitate comparison of yield estimates, paired bar graphs are presented for those species with thick "seed coats", indicating harvest yield range values with and without seed coats.

(a) Results of this study. Plants hand stripped, weighs represent clean seed. Range values 750 - 1500 kg/ha and 525 - 1050 when corrected for 30% seed coat weight.

(b) Containing ascaridole, which kills internal parasites, *C. ambrosioides* is cultivated and machine harvested in Carroll County, Maryland for its oil. Yields of 1000 pounds of seed per acre (1120 kg/ha) have been reported (Seeman and Wilson 1984:307).

(c) Hand stripping, cutting, and mass collecting (whole plants cut, bundled, dried, and threshed, with fruits then winnowed) harvesting methods employed. Yields ranged from 473 - 769 kg/ha, 331 - 539 kg/ha when corrected for seed coat weight (Seeman and Wilson 1984).

(d) Quinoa plants cut, dried, flailed, and fruit winnowed (Mass collecting method). Yields ranged from 504 - 1008 kg/ha (Elmer 1942).

(e) Mass collecting method (White *et al.* 1955).

(f) Mass collecting method, machine threshed (Johnson and McCamant 1986).

(g) Best seven of eight 1 m^2 plots. Hand stripping method employed. Kernals considered 55% of total achene weight. Yields ranged from 300 - 850 kg/ha, 165 - 468 kg/ha when corrected for achene shell weight (Asch and Asch 1978).

(h) A single stand occupying 25 ft^2 (2.29 m^2) harvested by hand stripping. Assuming achene "shells" accounted for 50% of total achene weight, harvest yield values of 1115 and 557 kg/ha (corrected for achene shell weight) were derived (Murry and Sheehan 1984).

(i) Martin and Leonard (1967:932) report that the 627 farms growing sunflower commercially in the United States in 1959 produced 623,000 bushels of seed from 25,732 acres, for an average yield of 24.2 bushels (576 pounds of seed) per acre (645 kg/ha). Assuming that achene shells comprise 46% of total achene weight, Asch and Asch (1978:314) derived a hulled clean seed yield value of 350 kg/ha. Martin and Leonard also reported typical yields of 784 - 1120 kg/ha for the major producing states (Minnesota and North Dakota), and noted yields of 2016 kg/ha from California. In addition they provide an average world production level of 979 kg/ha for the 17 million acres under cultivation in 1959. The average U.S. and World production levels (645 kg/ha and 979 kg/ha are employed to define a range estimate, with associated range estimate yield values for cleaned seed being 348 kg/ha and 528 kg/ha.

(j) Harvest yield estimates reported by Will and Hyde (1917) for Indian groups of the Upper Missouri during the 1860s and 1870s. Yields of 8 - 30 bushels per acre (unshelled) allowed a range estimate of 358 - 1344 kg/ha to be derived.

(k) Yields of 305 - 1254 kg/ha reported by robson *et al.* (1967:247). Assuming the fruit capsule (involcure) accounts for 50% of seed yield (Robson *et al.* 1967), a range estimate of 152 - 617 kg/ha was derived for clean seed.

(l) Hand stripped, clean grain, range estimate for optimum yield, rainy year (Zohary 1969).

Application of the same 30% reduction factor to two other modern thick testa chenopods (*C. ambrosioides* and *C. missouriense* results in harvest yield estimates of 784 kg/ha and 331-538 kg/ha respectively (Fig. 11). While *C. missouriense* falls at the lower end of the range of values obtained for *C. berlandieri*, *C. ambrosiodes* falls within the *C. berlandieri* range.

The modified range estimate of 525-1050 kg/ha for *C. berlandieri* is also quite comparable to harvest yield values reported for *C. quinoa*, the (testa-less) domesticated chenopod of South America. Elmer (1942) reports average yields of 504-10008 kg/ha, and White *et al.* (1955:535) present production figures of 493-896 kg/ha. Recent initial plantings of *C. quinoa* (varieties 407 and 407 black) in Colorado have resulted in yields of 331-805 kg/ha. Yields of 997 kg/ha were also obtained from 5 1m^2 plots of *C. quinoa* variety 407 grown at Vadito, Colorado in 1985 (Johnson and McCamant 1986). Optimal yields in the 3,000-5,000 kg/ha range for *C. quinoa* have also been reported or projected (Elmer 1942:21; White *et al.* 1955:535; Johnson and McCamant 1986).

Because the harvest yield range estimate for *C. berlandieri* presented here is so comparable to the large acreage cultivated harvest yield estimates available for *C. quinoa*,

it would not seem unreasonable to propose a harvest yield range estimate of 500-1000 kg/ha for prehistoric chenopod cultivated in the eastern woodlands of North America. This range estimate of 500-1000 kg/ha can in fact be considered rather conservative, if differential efficiency of harvest methods are considered. The cutting of either infructescences (cutting method) or whole plants (mass collection method) for subsequent drying and flailing for seed recovery have been shown to be more effective methods for harvesting *Chenopodium* than simple hand stripping (Seeman and Wilson 1984:308-309) in that they result in higher harvest yields. Mass collection is the harvest method employed today in South America by Quechua Groups (Elmer 1942, Gade 1970), and may also have been the preferred prehistoric method in the east, judging from the stored sheaves of *C. berlandieri* recovered from dry caves and rock shelters (Seeman and Wilson 1984:309).

Nevertheless, the proposed harvest yield range estimate of 500-1000 kg/ha for *C. berlandieri* is considerably higher than available harvest yield values for other pre-maize crop plants of the eastern woodlands. Asch and Asch obtained harvest yield values of 77-468 kg/ha from 8 $1m^2$ plots of *Iva annua* (1978:310). Murry and Sheehan (1984) report a harvest yield value of 1115 kg/ha for *Polygonum erectum*, based on the collection of a 2.29 m^2 stand. Assuming that the achene "shell" accounts for 50% of total knotweed achene weight, *P. erectum* would have a corrected harvest yield value of 557 kg/ha, at the lower end of the range proposed for *C. berlandieri* (Fig. 11). Similarly, employing the 1959 United States and world average production figures for sunflower (645 kg/ha, 979 kg/ha; Martin and Leonard 1967:932) to define a harvest yield range estimate, and assuming that the achene "shell" accounts for 46% of the total achene weight (Asch and Asch 1978:314), the corrected harvest yield range estimate of 348-528 kg/ha would only slightly overlap with that of *C. berlandieri*.

Against the backdrop provided by the comparatively lower harvest yield levels of other prehistoric eastern cultigens, the hand-stripped yields of *C. berlandieri* stands underscore both its value as an uncultivated prehistoric plant food source and its pre-eminent potential as a cultigen in pre-maize gardens and fields. The high harvest yield levels of present day uncultivated stands of *C. berlandieri* may help to explain why it was initially brought under domestication prehistorically, as well as its ubiquitous presence and abundance in otherwise regionally varible pre-maize plant husbandry systems of the Eastern Woodlands (Smith 1985a:52). Although having a far thicker seed coat than the prehistoric thin-testa domesticated form of chenopod that was cultivated prehistorically in the Eastern Woodlands, the present day wild-weedy form of *C. berlandieri* in the Eastern United States—the subject of this study—represents an appropriate analog for the prehistoric domesticate in that it retains a number of characteristics of domestication (simultaneous inflorescence, extended seed retention) that strongly suggest that it represents the weedy descendant of the prehistoric domesticate (Wilson 1981).

The seeds of *C. berlandieri* and other "starchy-seeded" crops have relatively low protein and fat content when compared to the "oily-seeded" crops—sumpweed and sunflower (Table 3)—and are high in carbohydrates. As a result, they have been considered to have been less important nutritionally than the "oily-seeded" annuals and to have been roughly comparable to maize in terms of food value. Analysis of the essential amino acid pattern of *C. quinoa*, however, has shown it to be exceptionally high in two essential amino acids (lysine and methionine), which make it extremely attractive as a source of protein and as a general source of human nutrition (White *et al.* 1955; Cusak 1984:23). Although it is yet to be documented, it is quite likely that *C. berlandieri* has a similar amino acid pattern to that of *C. quinoa*.

In addition to shedding light on its initial domestication and subsequent development as an important crop in pre-maize plant husbandry systems of the Eastern United States, the documented economic potential and likely nutritional qualities of *C. berlandieri* also invite a comparison between *C. berlandieri* and maize, which came to

TABLE 3.—*The Nutritive Value of Seed of Cultigens of the Prehistoric Eastern Woodlands of North America (Percent dry basis).*

Species	Protein	Fat	Carb.	Fiber	Ash
Starchy-seeded					
Goosefoot (a)					
C. berlandieri	19.12	1.82	47.55	28.01	3.50
Maygrass (b)					
P. caroliniana	23.7	6.4	54.3	3.0	2.14
Knotweed (a)					
P. erectum	16.88	2.41	65.24	13.33	2.34
Oily-seeded					
Sumpweed (c)					
I. annua	32.25	44.47	10.96	1.46	5.80
Sunflower (d)					
H. annuus	24.00	47.30	16.10	3.80	4.00
Tropical crops					
Maize (c)					
Z. mays	8.9	3.9	70.20	2.0	1.2
Squash (c)					
C. pepo	29.0	46.7	13.10	1.9	4.9
Bean (e)					
P. vulgaris	22.0	1.6	60.8	4.3	3.6
Quinoa (f)					
C. qu;inoa	12.5	6.0	72.5	5.6	3.4

(a) Asch and Asch 1985:361; (b) Crites and Terry 1984; (c) Asch and Asch 1978; (d) Watt and Merrill 1963; (e) Wu Leung 1961; (f) White et al. 1955.

dominate prehistoric eastern agricultural economies after A.D. 1000. Estimating prehistoric harvest yield values for maize is particularly difficult, due to the lack of documented analog situations involving non-hybrid maize cultivated and harvested without the benefit of fertilizers, draft animals, or machinery. Will and Hyde (1917:103, 108, 142) provide maize yield statistics for the 1860s and 1870s for Indian groups of the Upper Missouri area (South Dakota, North Dakota) which range from 8-30 bushels per acre (1867 Kansa, 19 bushels per acre; 1867 Yanktons, 30 bushels per acre; 1874 Sac and Fox, 20 bushels per acre; 1878 Kansa, 8 bushels per acre; 1878 Santee Sioux, 26 bushels per acre; 1878 average for Upper Missouri groups, 20 bushels per acre). While a bushel of shelled corn weighs 56 pounds (Martin and Leonard 1967:965) it is likely that the statistics extracted by Will and Hyde from historical records referred to bushels of corn on the ear. Since 70 pounds of corn on the ear yields 50 pounds of shelled corn (at a 15% moisture level—Martin and Leonard 1967), each "bushel" of the 1860s-1870s likely contained about 40 pounds of shelled corn. Employing this figure of 40 pounds of shelled corn per bushel, a range estimate of 358-1344 kg/ha can be derived from Will and Hyde's 8-30 bushel per acre range (Fig. 11). While acknowledged as being "no doubt

generous as an estimate of prehistoric yields", average production figures of 10-45 bushels per acre (shelled) have been reported for the pre-hybrid maize grown prior to 1925 in the Black Warrior River valley of west-central Alabama (Peebles 1978:402-403), providing a harvest yield range estimate of 627-2822 kg/ha. The Will and Hyde maize yield data for the northern Plains in the 1860s-1870s probably represents a closer approximation than the Black Warrior River valley values to the prehistoric Woodland levels of maize production in the eastern woodlands.

It will, of course, be difficult to establish with any degree of confidence the economic potential and actual yield of maize in the eastern woodlands during the A.D. 1000 - A.D. 1200 period of transition to maize agriculture. But yields in excess of 1000 kg/ha (about 17 bushels per acre) would have been necessary before maize would represent an attractive alternative to *C. berlandieri* in terms of harvest yield alone. Potential yield obviously was not the only factor influencing crop selection, and maize would likely have required considerably less commitment of time and energy to harvest than *C. berlandieri* during the critical fall period of intensive hunting of deer and collecting of wild plant resources. While maize cobs could be picked, stripped, dried, and stored, *C. berlandieri* would have required cutting and drying of whole plants, followed by flailing and winnowing prior to storage. The continued cultivation of *C. berlandieri* after A.D. 1200 as a secondary field crop within maize dominated field systems is not surprising, given its high yield and nutritional profile and the nutritional shortcomings of maize (Robson *et al.* 1976:246-247).

The obvious next step in pursuing the issue of harvest yield levels for *C. berlandieri* and the other prehistoric cultigens of the Eastern Woodlands is to grow them in relatively large cultivated stands in order to establish their economic potential in well controlled field plot settings. When combined with ongoing morphological and quantitative analysis of archaeobotanical assemblages of these early prehistoric cultigens, such modern experimental studies should provide substantial illumination of the nature and importance of pre-maize food production systems in the Eastern Woodlands of North America.

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NOTES

[1]Assignable to subsection *Cellulata* of the genus *Chenopodium* on the basis of its reticulate-alveolate pericarp, the species *Chenopodium berlandieri* subsumes an extensive geographical variety of wild, weedy, and domesticated forms of chenopod. Although still frequently refered to as a distinct species within subsection *Cellulata*, *Chenopodium bushianum*, a large fruited chenopod of the Northeast, Midwest, and mid-latitude Southeast, has been shown to have considerable genetic affinity with *C. berlandieri*, rather than being biologically distinct (Wilson and Heiser 1979; Wilson 1980:260).

As the systematics of eastern North American chenopods belonging to subsection *Cellulata* is revised, it is highly likely that *C. bushianum* will be relegated to subspecific taxonomic status within *C. berlandieri*. In anticipation of this reassessment and reassignment of *C. bushianum*, the species designation *C. berlandieri* is used throughout this article, even when geographical location and fruit size would suggest the species label *C. bushianum*.

[2]*Chenopodium* produces small indehiscent "seed-like" fruits, each consisting of a seed enclosed by a very thin adherent pericarp. Since this thin pericarp is the only thing distinguishing fruits from

seeds, the terms "seed" and "fruit" are used interchangeably. Winnowing was accomplished in a two step process. Collected infructescence material was first rubbed between the palms of the hands to dislodge fruits from attached perianths, and the material was then distributed along the top edge of an inclined cotton sheet. The lighter and more angular perianth, leaf and stem fragments would adhere to the sheet while fruits would roll down the angled sheet to be collected at its base.

Chemotaxonomie der Pflanzen, Bd. 7: Nachträge zu Band 1 und Band 2. [Plant Chemotaxonomy, Vol. 7: Addenda to Vols. 1 and 2] R. Hegnauer. Basel: Birkhäuser Verlag, 1986. Pp. 804. $278.91 (cloth).

This is the seventh volume in an immensely valuable series of reference books unequalled in any language. It and two planned future works attempt to update and expand upon the original six volumes, published between 1962 and 1973. Volume 7 supplements volumes 1 (algae, fungi, bryophytes, pteridophytes, and gymnosperms) and 2 (monocots). Volume 8 will augment the material on dicots contained in volumes 3-6 and provide indices for the entire set. Volume 9 will deal exclusively with legumes.

The set as a whole is a systematic, voluminous compilation of the available information on the chemistry of plant constituents. Despite the title, the primary emphasis of the series is on the chemical makeup of various plant taxa rather than on the direct application of chemical evidence in taxonomy. Various diagrams and discussions are presented highlighting diverse theories on the phylogenetic relationships among taxa, but this is done in a superficial way and does not represent the main thrust of the books.

The original volumes surveyed the plant (and fungal) kingdoms family by family, presenting a description of each taxon followed by a discussion of organic constituents which have been isolated from various species in each family. Organic acids, alkaloids, carbohydrates, lipids, and many more compounds are discussed in some detail. The pages are studded with numerous diagrams of chemical structures and tables listing the results of quantitative analyses. There are also frequent charts showing the interrelatedness of various compounds and outlining the biochemical pathways by which some compounds are synthesized.

Perhaps the strongest point of the series is the extensive encyclopedic referencing which will permit the reader to follow up on any line of information contained therein. Volume 7, for example, includes a 195-page annotated bibliography of works in English, French, German, Spanish, and several other languages, this in addition to the reference listing at the end of each chapter. Ethnobiologists seeking information on the makeup of plants used by native peoples can do little better than to start the literature search here.

Those of us fortunate enough to read German will gain even more from this useful reference work as a starting point from which to draw information on chemical botany. I am looking forward to the remaining volumes.

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GENERAL PLANT CATEGORIES IN THOMPSON AND LILLOOET, TWO INTERIOR SALISH LANGUAGES OF BRITISH COLUMBIA

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ABSTRACT.—Lillooet and Thompson, two closely related Salish languages of northwestern North America, are similar, though not identical, in their perception and encoding of major plant classes. Both lack free-standing terms for the "plant" kingdom, but have suffixes which denote "plantness" when incorporated in generic-level plant names. Both recognize nomenclaturally categories of "trees," "grasses," "low, herbaceous, broad-leaved plants," "mosses," "mushrooms" and "berries." Thompson also has a named category of "bushes," whereas this class is only incipient in Lillooet. In both languages there is an incipient, but unnamed class of "edible roots (and underground parts)."

The historical derivation of the category names is discussed in relation to suggested encoding patterns of Berlin (1972) and Brown (1984). A number of the major categories are at least partially defined by utilitarian features. The categories are not necessarily mutually exclusive. Most of the major categories have many recognizably distinct members which are unnamed at the generic level. Lillooet and Thompson major plant classes seem fluid and ambiguous compared with the "life forms" of Berlin and Brown.

INTRODUCTION

Folk biological classification has been a major focus of research by ethnoscientists over the past two decades, stimulated largely by the pioneering investigations of Brent Berlin and his colleagues (cf. Berlin 1972; Berlin, Breedlove and Raven 1966, 1973, 1974). On the basis of their research in folk biotaxonomies they have identified five universal ethnobiological ranks arranged hierarchically and containing mutually exclusive taxa. The most general, all-inclusive category is the "unique beginner" (ie., "plant," in the case of folk plant taxonomies). Categories of "life form" rank are the next most inclusive. They are described as being " . . . invariably few in number, ranging from five to ten, and among them are included the majority of all named taxa of lesser rank . . . Life form taxa are labeled by linguistic expressions which are analyzed lexically as *primary lexemes* . . . and may be illustrated by the classes named by words such as *tree*, *vine*, *bird*, *grass*, *mammal*, and so forth." (Berlin, Breedlove and Raven 1974:25-26).

Included within life form taxa are taxa of "generic" rank, and within some of these are contrast sets of "specific" taxa, which, in turn, may include "varietal" taxa. A sixth rank, comprised of "intermediate" taxa, is occasionally interposed between life form and generic taxa. However, these are said to be rare and not usually linguistically labeled (Berlin, Breedlove and Raven, 1974:26). The various taxonomic ranks, as perceived by

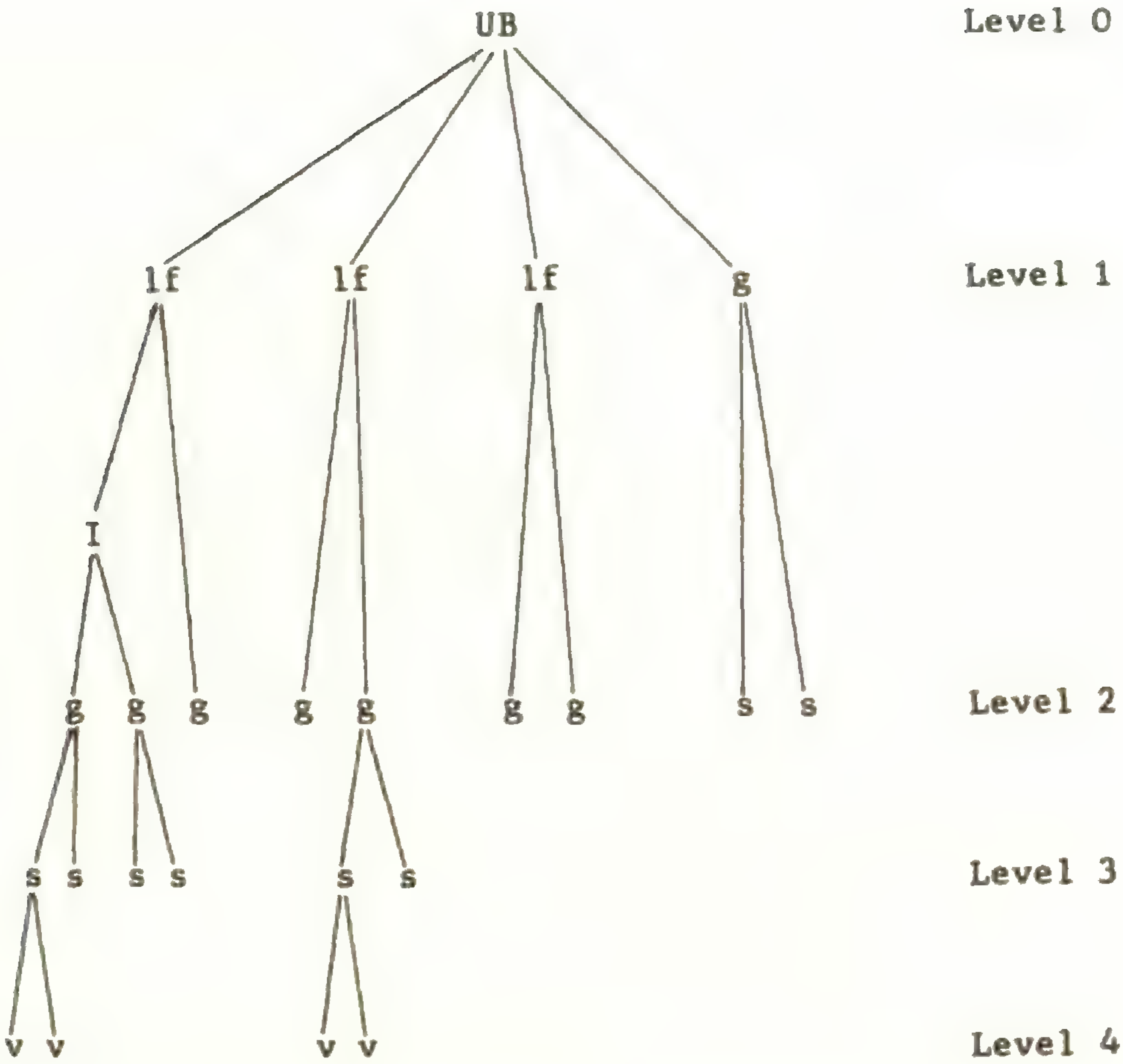

FIG. 1.—Schematic diagram showing hierarchical arrangement of folk biological categories as described by Berlin and his co-workers (after Berlin, Breedlove and Raven 1974:26). (UB - Unique Beginner; lf - life form; I - Intermediate; g - generic; s - specific; v - varietal.)

Berlin and his colleagues, are shown schematically in Fig. 1. "Life-form" classes in folk biology have been further investigated by Cecil Brown and his co-workers (cf. Brown 1977; Witkowski, Brown and Chase 1981; Brown 1984; Brown 1986). From surveys of folk classification in 188 languages for plants and 144 languages for animals, Brown has postulated a number of cross-language uniformities in the linguistic encoding of life form taxa (Brown 1984).

Some problems with the "life forms" and other biological taxonomic categories as perceived by Berlin and Brown and their colleagues have been pointed out by Hunn (1982) and Randall and Hunn (1984). Hunn and Randall argue that neither Berlin nor Brown adequately accounts for the influence of cultural importance of organisms on folk biological classifications. Furthermore, their descriptions, they maintain, do not reflect the real complexity of folk taxonomies, even at a general level.

In this paper I will describe and compare the most inclusive plant classes—those at the level of Berlin's "life form" and "unique beginner" ranks—within the linguistic and cognitive systems of Lillooet and Thompson, language groups of the Interior division of the Salish language family. Additionally, I will discuss the characteristics of Lillooet and Thompson general plant categories in relation to the universal features postulated by Berlin and Brown and their co-workers, with particular attention to the criticisms of these schemes made by Hunn and Randall.

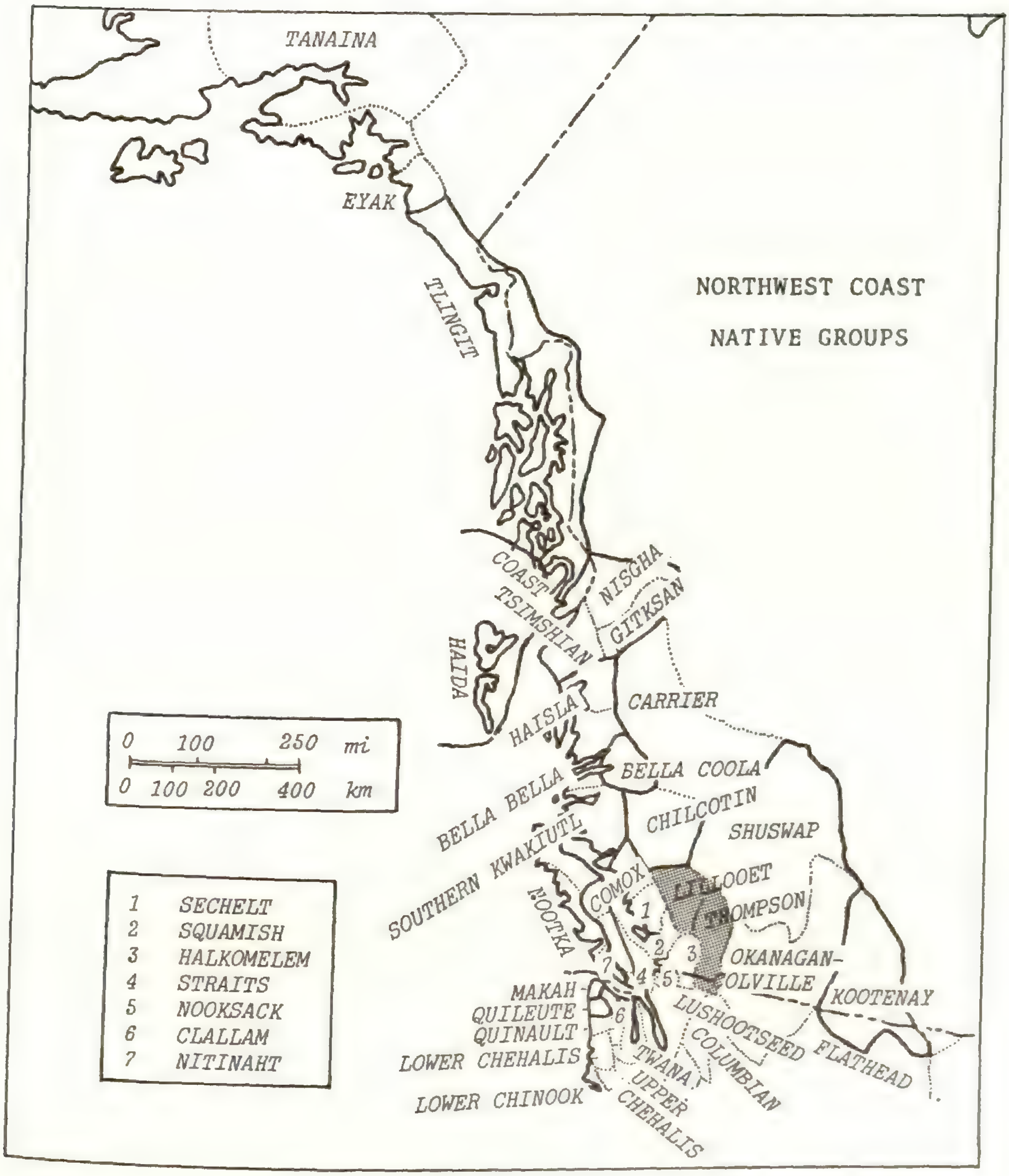

FIG. 2.—Map showing Lillooet and Thompson traditional territories.

This work is part of a broader study comparing many aspects of Lillooet and Thompson ethnobotany. These language groups are not only closely related linguistically, but also geographically, ecologically and culturally. Their geographical proximity is shown in Fig. 2. Their traditional territories extend over a broad and diverse area, encompassing the same range of biogeoclimatic zones. Hence, both groups have had access to the same, relatively diverse flora. Both languages are divided into "Upper" and "Lower" dialect regions, which correspond roughly with the ecological division between the hot, dry interior and the cooler, moister regions closer to the Pacific coast. Both groups are classed within the Plateau culture area, although both, especially their "Lower" divisions, exhibit influences of the adjacent Northwest Coast culture area. Both had tradi-

tional economies based on hunting, fishing and gathering. Except for native tobacco agriculture was not practiced until the advent of Europeans, although controlled burning for the maintenance of desirable habitats for certain food species was.

Data for this study were obtained through interviews with native speakers of Lillooet and Thompson conducted over a period of many years—since 1972 for Lillooet and 1973 for Thompson (see Appendix 1). A preliminary description of Lillooet folk plant taxa was included in Turner (1974). However, additional research and recent discussions of folk biological systems in the literature have contributed to an elaboration and refinement of the original descriptions of major plant categories in Lillooet in the present paper. Thompson folk plant classes were discussed in Turner *et al.* (1984).

Several linguists specializing in these native languages collaborated on this project, including Jan van Eijk, Randy Bouchard, Laurence C. Thompson, M. Terry Thompson and Steven Egesdal. Interviews were in English; virtually all the native elders speak English as well as their own language. However, plants were usually referred to by their native, rather than English, names, or simply by using growing or freshly picked specimens of various plants as samples to be discussed. General plant taxa were identified and inventoried by means of informal conversations about plants, and through discussions of applied terminology and more formal questions about the relationships and attributes of individual plant species and folk generic level taxa.[1]

The Lillooet and Thompson communities, like other native communities in British Columbia, have become largely assimilated linguistically and culturally to the dominant Euro-american English-speaking population. Though many aspects of native culture remain, these have been significantly influenced by the "White" culture. Few members of the younger generations of native people are able to speak their traditional languages fluently, and even elders may not recall some of the more obscure or specialized vocabulary. Inevitably, cognitive systems have changed. Nevertheless, their underlying structure, as reflected in language, remains. Furthermore, the changes themselves, and the ways in which new items and concepts have been incorporated, are also a valid and productive subject for research. In the case of folk plant taxonomies, studying how existing folk taxa have been altered and new taxa developed with the influx of exotic plants and plant products can reveal insights into the evolution of folk classification systems.

THE "PLANT" CONCEPT IN LILLOOET AND THOMPSON

Typical of northwestern North American languages (cf. Turner 1974; Turner and Bell 1971, 1973; Turner, Bouchard and Kennedy 1981; Turner and Efrat 1982; Turner *et al.* 1984) and in accord with Berlin's general principles (Berlin 1972:78; Brown 1984:4) neither Lillooet nor Thompson has a free-standing term denoting any and all members of the plant kingdom. "Plant" is nevertheless a psychologically valid category in Lillooet and Thompson. Our native consultants showed no hesitancy in discussing plants as a discrete group, to the exclusion of animals and inanimate objects. One might argue that the "plant" concept was acquired through contact with English-speaking peoples. However, the existence of suffixes and descriptive term in these languages with semantic ranges encompassing a broad spectrum of "plants" suggests otherwise.[2]

In both languages, there is a suffix which may be variously translated as "plant," "bush" or "tree." In Lillooet the suffix is ***-aẓ́,*** alternating with ***-ɬəp*** (var. ***-ɬp***) or ***-alp*** in terms most likely borrowed from neighboring languages. This latter suffix is cognate with the Thompson suffix ***-elp*** and its variants. Examples of plant names containing these suffixes are shown in Table 1. In some instances, as with fruiting plants, the "plant" suffix is added to the name for the fruit or other culturally significant plant part to form the name for the whole plant [e.g. Lillooet ***q̓an*** - black hawthorn[3] fruit, ***q̓án-aẓ́*** — black

TABLE 1.—*Examples of plant names in Lillooet and Thompson incorporating "plant" suffixes.*

Lillooet*:

Plant name	English gloss	Corresponding species
pún-ɬəp		Rocky Mountain juniper
cík̓əkt-aẓ́		common juniper
mə̓lin-ɬəp		subalpine fir, grand fir
c̓k-aẓ́	'pine-seed-plant'	whitebark pine
qʷlít-aẓ́ (P)	? 'pitch-plant'	lodgepole pine
χ̓əmq̓́-áẓ́		western yew
təx̌ʷʔac-áẓ́/təx̌ʷʔac-áɬp	'bow-plant'	western yew
ləx̌-áẓ́ (F)		"cut-grass"
nix̌-ənx̌-aẓ́ (P)		'cutting-plant'
χ̓áqʷam-aẓ́		"timbergrass"
sík̓-ɬəp		vine maple
káw-kw-aẓ́		big sagebrush
t̓áqʔ-aẓ́		salal
pácʔ-aẓ́	'digging-stick-plant'	oceanspray

*Unless otherwise specified, names occur throughout the languages; in Lillooet, F denotes Fraser River dialect only, P, Pemberton dialect only.

Thompson:

Plant name	English gloss	Corresponding species
p̓ə̓sk̓eʔ-éɬp	'hummingbird-plant'	Indian paintbrush, shrubby penstemon
məckʷ-éɬp	'blackcap-plant'	blackcap
s-q̓ʷoq̓ʷy̓ep-éɬp	'strawberry-plant'	wild strawberry
q̓ʷuys-éɬp	'silverberry-bark-plant'	silverberry
ʔik-eɬp-éɬp	'kinnikinnick-plant-plant'	twinflower
sc̓um̓-mn-éɬp	'sucking-substance-plant'	orange honeysuckle
y̓əɬyeɬiʔtn̓-éɬp	'cough/cold-plant'	pussytoes
sp̓ec̓n-éɬp	'twine-plant'	Indian hemp
qʷn-éɬp		Indian hellebore

TABLE 1.—*Examples of plant names in Lillooet and Thompson incorporating "plant" suffixes.* (continued)

Thompson:

Plant name	English gloss	Corresponding species
q^{w}iʔt-élp		lodgepole pine
p̓əp̓uʔn̓-élp	'flatulating-plant'	pasture wormwood, rabbit-brush
púnłp		Rocky Mountain juniper, sometimes common juniper
c̓q̓-álp	'sticky-plant'	Douglas-fir
x^{w}ik^{w}estn-élp	'scrubber-plant'	western hemlock
χ̓áq^{w}-elp		pine grass

hawthorn bush; ***(s)psúṣ*** — bitter cherry fruit, ***psúṣ-aẓ̓*** — bitter cherry tree; or, alternately ***sʔiẇx^{w}***—bitter cherry bark (when removed from tree for use in wrapping implements and basket decoration), ***ʔiẇx^{w}-aẓ̓*** — bitter cherry tree]. In other cases, the suffix is applied to a term which indicates an application of the plant or some other significant association (e.g. Lillooet ***pácʔ-aẓ̓*** — oceanspray; cf. ***pácaʔ*** 'digging-stick'[4]). In a few terms, the stems are unanalysable (e.g., Lillooet ***cátaw-aẓ̓*** — red cedar, ***cáx̌-aẓ̓*** — Sitka spruce; Thompson ***q^{w}n-élp*** — Indian hellebore). In Lillooet, about 50 percent of all plant names of generic rank include the "plant" suffix ***-aẓ̓*** or the equivalent borrowed suffixes, ***-łəp/-álp*** (Turner, 1974:31). In Thompson, about 20 percent of the plant names incorporate the "plant" suffix ***-élp***, either optionally or mandatorily. It is notable that the names including this suffix pertain to a broad range of plants—mostly trees and shrubs, but also denoting some low herbaceous plants such as pine grass ("timbergrass") and wild strawberry. The existence of a "plant" suffix is widespread in northwestern North American languages.[5]

When additional terminological data are considered, the reality of the "plant" category is reinforced. For example, there are numerous morphological terms for different parts of plants that are exclusive to the domain. These include words for "leaf," "root," "branch," "stem," "shoot" and "seed" in both Lillooet and Thompson (J. van Eijk, pers. comm. 1985; Turner *et al.* 1984). These terms are applied to plant species the names of which include the "plant" suffix, to those which do not, and to plants which are unnamed. Hence, the existence of a covert "plant" category is implied by the range of application of anatomical terminology. Furthermore, as discussed later, many of the generic level plant names in Thompson which do not contain the "plant" suffix incorporate terms for major plant categories, especially those for "low herbaceous, broad-leaved plants" and "flowers." This reinforces at a basic level the notion of these plants belonging to larger groupings and ultimately, by virtue of overlapping major classes, to an all-inclusive category, "plant."

Are fungi and non-vascular plants such as mosses and lichens considered as "plants" in the traditional Lillooet and Thompson worldview? Contemporary native consultants

have no difficulty in categorizing them as such, but in neither language is there any common linguistic element, such as the "plant" suffix, linking these organisms to other plants. Because they grow and do not move around, their "plantness" may have been perceived even in aboriginal times.

"LIFE FORM" LEVEL PLANT CATEGORIES

Below the "plant" class, the most inclusive labeled plant categories for Lillooet and Thompson are several roughly equivalent to the "life forms" of Berlin and Brown. These are listed in Tables 2 and 3. As might be expected, the names for and ranges of the major plant categories of Lillooet and Thompson are quite similar.

In Lillooet, the major named categories include: "trees (especially coniferous trees)," "grasses and grass-like plants" and/or "hay," "low, herbaceous broad-leaved plants of little cultural significance," "flowers," "berries/fruits," "mosses and moss-like plants" and "mushrooms and fungi." Additionally, "bushes" are generally recognized as a discrete category, but are not really named as such. Plants with culturally significant (ie., edible) roots and underground parts are also conceptually discrete, even though there is no inclusive Lillooet term for these (Turner 1974:41).

TABLE 2.—*General plant classes in Lillooet.*

Lillooet name	Approx. English equivalent	Examples of incl. taxa
s-ɣáp ('that which is upright')	"tree"	Douglas-fir; bitter cherry; cottonwood; ponderosa pine
(s)lə́qəm/s-c̓ə́pəz̓	"hay"/"grass"	bunchgrass; timothy; giant wild rye
s-wa?p-ú̓l̓m̓əxʷ ('ground-growth/hair')	"low, herbaceous, broad-leaved plants of low cultural importance"	millein; plantains; lamb's quarters; twinflower
s-p̓ə́q̓əm (cf. ***p̓aq̓*** 'white/light-coloured')	"flowers"	self-heal; wild rose; *Penstemon serrulatus*
s-q̓ʷəl ('ripe/cooked'); ***-usa?*** ('round/oval object')	"berries/fruits"	saskatoon; salal; black huckleberry; thimbleberry
pə́?səm	"mosses and moss-like plants"	*Rhytideadelphus triquetrus; Dicranum scoparium; Cladina* spp.; *Selaginella wallacei*
s-q̓əm̓s (P);	"mushrooms and fungi"	pine mushroom; "cottonwood mushroom"; shaggy mane; puffballs
unnamed	"edible roots and underground parts"	tiger lily; chocolate lily; water parsnip; wild onions

TABLE 3.—*General plant classes in Thompson.*

Thompson name	Approx. English equivalent	Examples of incl. taxa
s-γêp	"large trees"	Douglas-fir; red cedar; pines; cottonwood
múyx	"tall bushes and small deciduous trees"	mountain alder; Rocky Mountain maple; shrubby willows; oceanspray
s-yîqm	"grasses/hay and grass-like plants"	bunchgrass; reed canary grass; giant wild rye; "timbergrass"
s-tuyt-ûyṁxʷ ('ground-growth/hair')	"low, herbaceous, broad-leaved plants of low cultural importance"	stonecrops; plantains; vetch; lamb's quarters
s-ṗâq̓m	"flowers"	calypso; buttercup; arnica; garden flowers
s-q̓ʷîyt/ ***s-q̓ʷit-ɛ̂łp/*** ***-ûse?*** ('round-object')	"berries/fruits"	huckleberries; saskatoon; soapberry; strawberry
qʷzêm	"mosses and moss-like plants"	*Hylocomium splendens*; *Eurhynchium oreganum*; *Lobaria* spp.; *Lycopodium clavatum*
q̓âṁes (Lower)/ ***məx̣̓qî?*** (Nicola)	"mushrooms and fungi"	pine mushroom; "cottonwood mushroom"; shaggy mane; oyster mushroom
(q̓əc̓)q̓əc̓-usnîni ('woven-as-it-grows')	"large vines"	white clematis; orange honeysuckle (sometimes); grape; English ivy
unnamed	"edible roots and underground parts"	wild onions; yellow avalanche lily; spring beauty; bitterroot; balsamroot

In Thompson, the major named categories are: "large trees (especially coniferous trees)," "tall bushes and small deciduous trees," "grasses and grass-like plants," "low, herbaceous broad-leaved plants of little cultural significance," "flowers," "berries/fruits," "mosses and moss-like plants" and "mushrooms and fungi." "Vines" as a category in Thompson apparently originally included only two or three species, but has been extended. A category, "roots and underground parts," though unnamed, is also recognized. Each of these categories will be discussed in some detail, both in respect to its counterparts in related languages and to its corresponding "universal life form" status.

In both Lillooet and Thompson, the "tree" category comes closest to exhibiting the features of a life form (cf. Brown 1982:5-6). It is subordinate to the unique beginner and is labeled by a "primary lexeme" (Berlin, Breedlove and Raven 1973:215). This lexeme is complex, however, since it is analyzable, expicitly referring to the standing form of the class members (lit. 'that-which-is-put-upright') (J. van Eijk, pers. comm., 1985; Thompson and Thompson, in press; see also Table 4). The "tree" class in each language is polytypic, including a substantial number of "generic" taxa which are mutually exclusive and mostly labeled by primary lexemes, many of which are analyzable. It is defined, apparently, by a small set of morphological features (ie., large, upright, woody), and can be said to be "general purpose" (Brown 1982:14). However, the "tree" category in Thompson and Lillooet did not follow the most common development pattern as described by Berlin (1972:71).

One might suppose, in looking at Lillooet in isolation, that the "tree" category developed through expansion of reference, since, in Lillooet, Douglas-fir (Fig. 3), a highly salient species, is called ***s γap-ʔúl*** 'real/original tree.' One could envision the development of the "life form" name from a generic based on the premises of Berlin (1972:71) and Brown (1984:60).

However, when terms for Douglas-fir in neighboring Interior Salish languages are considered, the development of the more general life form name through expansion of reference in Lillooet seems unlikely. The Thompson, Shuswap and Okanagan-Colville terms for "tree" are related, and in all three cases, as well as in Columbian, where the 'tree' term is unrelated, their names for Douglas-fir are different and unrelated to their terms for "tree" (see Table 4). In view of this, as well as the fact that the term for "tree" in both Lillooet and Thompson is analyzable and implies an upright habit (as it does in Sahaptin, Flathead and Columbian, among other languages—Randall and Hunn 1984:341, Hart 1974:36; D. Kinkade, pers. comm., 1986), it seems more probable that the Lillooet term for Douglas-fir was derived from the "tree" term, through restriction of reference.[6] This would imply that the "life form" term "tree" is chronologically older

FIG. 3.—Douglas-fir, the "type" for the "tree" category in Lillooet.

TABLE 4.—*"Tree" terms and names for Douglas-fir in British Columbia Interior Salish Languages.* *

Language	"Tree"	Douglas-fir	Reference
Lillooet	***s-γâp*** ('that which is upright')**	***s-γap-ʔûl*** ('real/original tree')	J. van Eijk, pers. comm. 1985
Thompson	***s-γêp*** ('upright/pole')	***c̓q̓-ât̓p*** ('sticky-plant')	Turner et al. 1984
Shuswap	***tsegâp*** ***(cəγâp)***	***tsk'alhp*** ***(c̓q̓-êɬp)***	Palmer 1975:44, 52
Okanagan-Colville	***tsegîp/tsiyîp*** ***(cəγîp/ciyîp)***	***tsk'ilhp*** ***(c̓q̓-iɬp)***	Turner, Bouchard and Kennedy 1981: 156

*Additionally, D. Kinkade (pers. comm., 1986) provides the following information for Columbian, an Interior Salish language of Washington. The singular term for tree is ***ʔac-p̓âx̌*** ('single upright object'), the plural term is unrelated: ***ʔas-c̓ǎlc̓əl*** ('plural upright objects'). The latter also sometimes means 'brush, bushes.' The Columbian term for Douglas-fir is ***c̓q̓-ât̓p,*** cognate with Thompson, Shuswap and Okanagan-Colville.

Cf. root ***γip* 'to grow,' ***γəp*** 'to stand up,'; hence ***s-γáp*** means 'the grower,' or 'the standing-up one' (J. van Eijk, pers. comm. 1986).

than the "generic" term "Douglas-fir" in the Lillooet language. However, that does not mean that the category "tree" is older than the generic-level category "Douglas-fir." Rather, it is likely that Douglas-fir had an earlier name in Lillooet, perhaps cognate with the other Interior Salish terms, and the present name, 'real/original tree,' replaced the earlier name.

There is an additional piece of evidence to support the second supposition. Lillooet has a term, ***c̓q̓áq̓ləp,*** which is a reduplicated form of ***c̓q̓-at̓p,*** a form cognate to the Thompson name for Douglas-fir. While ***c̓q̓-ât̓p*** itself does not occur in the Lillooet vocabulary, the presence of its diminutive form, meaning "young tree" and in particular "young Douglas-fir,"[7] implies the former existence of the more basic term (J. van Eijk, pers. comm. 1986). Van Eijk postulates that the term ***c̓q̓-ât̓p*** may have fallen into disuse in Lillooet as a result of a word taboo, to be supplanted by the "real tree" epithet. Douglas-fir plays an important role in Lillooet and Thompson ritual and religious traditions (Turner 1986).

Incidentally, the Lillooet and Thompson application of "tree" has some ambiguity. Most of the core species in the taxon, those that would be exemplary, are large coniferous species, but on closer questioning most native speakers would include black cottonwood, which grows throughout Lillooet and Thompson territory, and red alder, trembling aspen and broad-leaved maple, where these occur. Marginal species would be Rocky Mountain maple and mountain alder, which might also be categorized as large bushes.

The "bushes and small deciduous trees" category in Thompson is perceptually more distinct than the corresponding Lillooet category. In the former it is named (***múyx***), whereas in the latter it is only implied by application of the cognate term ***mûlx*** ("stick")

(also applied to "trees"), and its reduplicated plural forms, ***məl-múlx***, or ***məl-məmi̓əx***, ("bushes/woods/underbrush") to dense thickets of any type of bushes. The Thompson plural form, ***mi-múyx***, also pertains to "woods/bushy places," but the Thompson ***múyx***, unlike Lillooet ***múlx***, seems to be used as a class name and is actually incorporated into the generic-level names for some shrubs. It is possible, however, that its meaning has simply tended to conform with "bush" within historical times, as a form of semantic convergence with English.[8] In a similar vein, there also may well be a tendency in Lillooet for the term ***múlx*** "stick" to assume the semantic role of a class name for "bush." Future speakers of Lillooet, if the language persists, may use it as such.

Like the "tree" category, the Thompson ***múyx*** category poses an interesting problem in the origin and evolution of general categories. In the Okanagan-Colville and Shuswap languages, the cognate terms, ***múlx*** and ***múlux*** (or ***múlx***) respectively (Turner, Bouchard and Kennedy 1981:134; Palmer 1975:68; Kuipers 1974:150), are generic names for black cottonwood. The term ***múlx*** is also used for cottonwood by the Nicola Valley Thompson, having been borrowed from Okanagan-Colville (Mabel Joe, pers. comm., 1984). In a number of southwestern American languages, the name for "cottonwood" is polysemous with the name for "tree" (cf. Trager 1939), but in Interior Salish, the relationship is more complicated because it transcends the boundaries of related but distinct languages, as well as the boundaries of two different major taxa, "tree" and "bush." Perhaps this situation reflects an original perceptual dichotomy between "evergreen woody plants" and "broad-leaved woody plants," rather than between "tree" and "bush." Expansion of reference—from "cottonwood" to "bush"—may well have occurred but if so, this semantic shift must have taken place in proto-Salish, while these languages were diverging. It seems equally plausible that the names for black cottonwood in Okanagen-Colville and Shuswap developed through restriction of reference from a proto-Salish term for "wood/stick" in the same manner as the Lillooet name for Douglas-fir must have been derived from the more general term of "tree." Brown (1984:60) contends that in most languages the life-form name for "tree" derived historically from a term for "wood" or "firewood", which would have been more salient than "tree" in early cultures. In this case, "stick" may have had higher salience than "cottonwood" in a proto-Salish language, and the latter term have been derived from the former. Such a conjecture would be difficult to prove.

The "grasses and grass-like plants" categories in Lillooet and Thompson, like the "bush" classes, are subtly different, though roughly equivalent. They, too, seem to be diverging semantically. Most Lillooet speakers of the Fraser River dialect clearly distinguish ***s-ləqəm*** "hay" from ***s-c̓əpəẓ*** "grass" (J. van Eijk, pers. comm., 1985).[9] (In Fraser River Lillooet, ***ləqm-am*** means 'to make hay.') However, in the Pemberton Lillooet dialect, though ***s-ləqəm*** is glossed primarily as "hay," it is used for "any grass/grass-like plant" since no other term for "grass" is known. Still, ***s-ləqəm*** is now exemplified by introduced hay crop species such as timothy and red-top, as viewed by at least one Pemberton speaker (A. Peters, pers. comm., 1984). In Thompson, the cognate term ***syiq-m*** applies to "grass" or "hay" interchangeably.[10]

Bluebunch wheat grass ("bunchgrass"), a species of high salience in the dry Interior of British Columbia, is usually considered the prototype for "hay" and "grass/hay" respectively in Fraser River Lillooet and Thompson (Fig. 4). Traditionally it is known as excellent fodder for deer, and in historic times, has been valued as a hay crop and grazing species for horses and cattle (Steedman 1930:515; Turner, Bouchard and Kennedy 1981:53). It has been called ***s-ləqəm-ʔúl*** ('real/typical hay') in Fraser River Lillooet, but younger speakers of the language today (eg., Desmond Peters, pers. comm., 1985) apply this term to alfalfa and the sweet-clovers, which are not grass-like at all; both are in the legume family. In Thompson, the cognate form ***s-yiqmʔúy*** ('real/typical grass/hay') also refers to bluebunch wheat grass. This species does not occur in the coastward parts

FIG. 4.—Bluebunch wheat grass, or "bunchgrass," the original "type" for "hay" and "grass/hay" in the interior dialects of Lillooet and Thompson respectively. Today its role is largely supplanted by sweet-clovers, alfalfa and hay crop grasses.

of the Lillooet and Thompson territories and was not known to the Pemberton Lillooet people.

"Bunchgrass" is also known as 'real hay' to some Shuswap speakers (Deadman's Creek) (Kuipers 1974:166), and in Okanagan-Colville it has two names, one cognate with Shuswap "hay/grass," and the other polysemous with the general Okanagan-Colville term for "grass, hay or grass-like plant," (see later discussion on "low . . . broad-leaved plants . . ."') (Turner, Bouchard and Kennedy 1981:52-53). The various names for "grass," "hay," and "bluebunch wheat grass" in the four British Columbia Interior Salish languages are shown in Table 5.

What is the origin of the general class names for "hay" in Lillooet and "grass/hay" in Thompson? Have they developed in the manner proposed by Berlin (1972:71) as a common method for derivation of general taxon names, through expansion of reference from the "generic" name of a highly salient species (ie., "bluebunch wheat grass")? Or, conversely, did they exist prior to the name for the generic "type"? In this case, I would suggest the former developmental sequence, based on the existence of the name 'real/typical grass/hay' for bluebunch wheat grass in three out of four of the British Columbia Interior Salish languages, and on the specific application of the cognate term for Shuswap 'grass/hay' to bluebunch wheat grass in the fourth language, Okanagan-Colville. A tentative sequence of encoding is proposed in Fig. 5.

In Fraser River Lillooet, both the "hay" and "grass" categories are largely "empty," since each includes only a very few members named at the generic level. Hence, neither would qualify as a *bona fide* life form according to the criteria of Berlin and Brown. In Thompson, there are several (approximately ten) named terminal taxa incorporated in "grass/hay," hence it qualifies as a true life form.

TABLE 5.—*"Grass" and "hay" terms and names for bunchgrass in British Columbia Interior Salish Languages.* *

Language	"Grass"/"Hay"	Bluebunch wheat grass	Reference
Lillooet	"hay" - ***s-lə́qəm***	***s-ləqəm-ʔúl*** (F) ('real hay')	J. van Eijk, pers. comm. 1985
	"grass" ***s-ć̓əpəẓ̓***		
Thompson	"grass/hay" - ***s-yīqm***	***s-yīqm ʔúy***	Turner et al. 1984
Shuswap	"hay/grass" - ***s-t̓yē?;*** or ***-ēst̓ye*** (suffix)	***s-t̓ye?-ú?y*** (DC)	Kuipers 1974:63, 166
Okanagan-Colville	"grass/hay" ***swupúla7xw*** ('ground-growth/hair')	***st'iyī7;*** or sometimes ***swupúla7xw***	Turner, Bouchard and Kennedy 1981: 52

*F = Fraser River Lillooet dialect; DC = Deadman's Creek Shuswap dialect.

The category, "low, herbaceous, broad-leaved plants of little cultural importance," is more or less equivalent in Lillooet and Thompson, although in Lillooet the term ***s-wa?p-úlm̓ex^{w}*** is used only in a general way, while in Thompson the corresponding term, ***stuyt-úym̓x^{w},*** is often incorporated in the names for particular kinds of plants (see Table 6 for examples).

In each language, this category appears to be a residual taxon, encompassing a wide variety of relatively short, non-woody, leafy species of low cultural significance. It is tempting to equate this category with the English folk taxon, "weed," as did the early ethnographer James Teit, who in his unpublished field notes on Thompson (1896-1918), recorded under lamb's quarters: ***"stuwitu'imax . . . "*** or ***"stuwitu'imax a sáma"*** ['whiteman's ***stuyt—úym̓x^{w}***'] . . . weeds introduced by whites with no particular name or use especially annuals . . . " Contemporary native elders also generally translate ***s-wa?p-úlm̓əx^{w}*** (Lillooet) and ***stuyt-úym̓x^{w}*** (Thompson) as "weed." Indeed, one Lillooet speaker defined the term ***s-wa?p-úlm̓əx^{w}*** as " . . . just any weed in a garden . . . any type of mixed plants, or low plants, it's just like [that] speargrass[11] . . . whatever comes in the garden." (Desmond Peters, pers. comm., 1985). However, the term did not, at least originally, have the same negative connotation that "weed" has to many non-Indians,[12] or that the GRERB ["grass-plus-herb"] category has in some other languages where its name derives from "rotten," "litter," "garbage" or "filth" (Brown 1984:63). Here, it is more a term of convenience, defined linguistically by physiognomic characteristics (ie., 'ground-growth') and applied as a sort of "catch-all" mostly for plants not important enough to have special generic-level names.

In each language, but especially in Thompson, the category encompasses many indigenous plants; originally, of course, it would have applied *only* to indigenous species. Teit's application of the modifer "whiteman's" to the category is indicative of a recognition in Thompson of both native and introduced ***stuyt-úym̓x^{w}***. With the advent of gardening, farming and ranching among the Lillooet and Thompson, coupled with a reduction

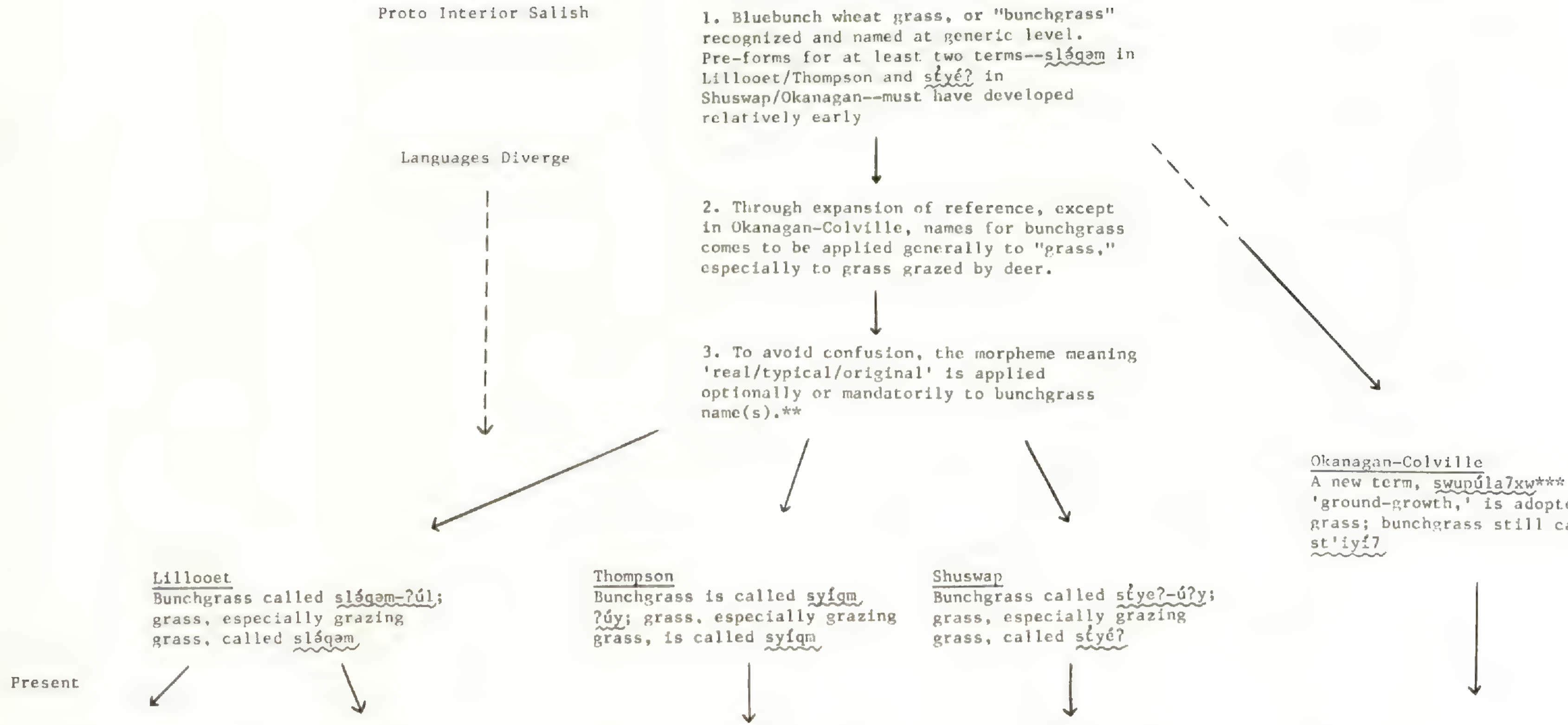

FIG. 5.—Proposed sequence of encoding of the Lillooet and Thompson general terms for "grass" and/or "hay."

NOTES (For Fig. 5)

*This is only one suggested series of derivations; more strenuous linguistic analyses would be required before this could be considered anything but very tentative.

J. van Eijk (pers. comm. 1986) notes that "the Lillooet form of this morpheme, *-ʔūl,*** is the original one, whereas Thompson (which generally shifted Proto-Salish ***l*** to ***y***) shows a later development. Shuswap, which like Lillooet generally retained Proto-Salish ***l,*** has unexpectedly ***-ʔūy.*** The Shuswap form is thus possibly a borrowing from Thompson. The problem of development of plant taxonomies thus interesects with the problem of phonological developments and borrowing. It is also difficult to say whether in Thompson the suffix ***-ʔūy*** was added to ***s-lə̄qəm*** (the ancestor of ***syīqm***) and that the entire form ***s-ləqəm-ʔūl*** shifted to ***syiqm-ʔūy*** . . . or whether ***-ʔūy*** was added to bunchgrass' after this form had already changed to ***syīqm.***"

D. Kinkade (pers. comm. 1986) notes that in Columbian Interior Salish, the related form, ***swpūl̓əx^{w}, also pertains to "grass," and is generally glossed 'short grass,' although one speaker said it was particularly 'bunchgrass.' It has also been glossed as 'wild hay.' The Columbian term ***stl̓iyaʔ*** refers to 'hay, tall grass.'

of wilderness activities such as hunting and gathering, introduced "weeds" have certainly gained in salience and wild 'ground-growth' became less salient, so that there has been a tendency for the class to conform with our "weed" class. Even today, though, the conformity is not exact, at least among the more traditionally minded native speakers. Annie York, a major Thompson consultant, for example, refers to ***stuyt-úym̓x^{w}*** very often in her discussions of low, herbaceous plants. She sometimes applies descriptive names such as ***ʔes-wəlwīwl tək stuyt-úym̓x^{w}*** ('having-little-fringe ground-growth'—for parsley fern) to such plants. She even incorporates the term in names for plants which are useful medicines, such as two varieties of rattlesnake plantain [e.g. ***ʔes-n-cəlcl-īlkn (tək stuyt-úym̓x^{w})*** 'little-humpbacked (ground-growth')—for a wide-leaved variety].[13]

It is notable that in Okanagan-Colville and Flathead, the equivalent "ground-growth" terms have assumed the semantic role of "grasses" (cf. Okanagan-Colville ***"swupúla7xw"*** (***swupúlaʔx^{w}***) 'ground-hair/growth' for "grass, general" and Flathead ***supúʔlex^{w}*** 'hair on the earth' for "grass"—Turner, Bouchard and Kennedy 1981:52; Hart 1974:34). In the latter language, however, a "weed" category exists which was derived from the "grass" category: ***ćes.upúʔlex^{w}*** 'bad hair on the earth' (Hart 1974:38). In Shuswap, the "weed" connotation for the equivalent term, ***"swupúlexw"*** (***swupúləx^{w}***), is retained (Palmer 1975:44).

The "flowers" category in each language is similar to the "ground-growth" category. It is somewhat of a "catch-all," generally applying to herbaceous species with conspicuous blossoms. It seems more or less equivalent to the English folk category "wild flowers," but with garden flowers also being included at the present time. If plants in the "flower" category are named more specifically, it is usually by some descriptive term (e.g., Thompson 'blue flower' for wild blue violet; 'creek flower' for rein orchid). The only "wild flower" in either language which has its own primary lexemic name is wild rose, called ***(s-)qəl̓q*** (bush — ***s-qəl̓q-áż)*** in Lillooet, but this term also applies to wild rose hips, and some apply it exclusively to the latter, calling rose flowers ***p̓áq̓əm s-qəl̓q-áż*** (lit. 'flowers of the rose bush') (Edith O'Donaghey, pers. comm., 1985).

The "flowers" category actually overlaps somewhat with the "ground-growth" category, as can be seen by the optional application of the general terms in the names of some plants in Thompson. For example, in several instances, Annie York used ***stuyt-úym̓x^{w}*** and ***s-p̓áq̓m*** interchangeably in naming some herbaceous plants that also had conspicuous flowers. Not knowing a "real" name for wild bleeding-heart, for instance, she applied a translation borrowing from the English name. She used both ***s-x^{w}ak^{w}***

TABLE 6.—*Some examples of generic-level names incorporating general class terms in Thompson (after Turner et al. 1984).*

Thompson Term	English gloss	Species referred to
ʔes-ɬəqlâq tək stuyt-úym̓xʷ	'clumped-here-and-there ground-growth'	licorice fern
ʔesnc̓əl̓c̓l̓-il̓kn (tək stuyt-úym̓xʷ	'little-humpbacked ground-growth'	rattlesnake plantain (wide-leaved variety)
ƛ̓əxʷƛ̓xʷáp peɬ stuyt-úym̓xʷ	'paralysed-person's ground-growth'	Canada goldenrod
(ʔe)s-pəspâs peɬ s-yīqm	'swamp grass'	*Carex* sp.
qəpqêp tək sp̓áq̓m	'soft flower'	western anemone
scẃêẃxʷ peɬ sp̓áq̓m	'creek flower'	rein orchid
q̓əpq̓əpkʷlê peɬ mûyx (OR ***q̓əpq̓əpkʷleh-êɬp)***	'"click-click" bush' (OR '. . .-plant')	highbush cranberry
xʔ-úym̓xʷ peɬ mûyx (a descriptive name)	'upland bush'	white-flowered rhododendron

xʷâkʷ kʷukʷ tək stuyt-úym̓xʷ (lit. 'heart ground-growth') and ***s-xʷakʷxʷâkʷ kʷukʷ tək sp̓áq̓m*** (lit. 'heart flower'). In one case, for an alternate name for buttercup, she applied these two general terms simultaneously: ***ʔes-kʷəlkʷlōʔ tək stuyt-úym̓xʷ e s-p̓áq̓ms*** (lit. 'little-yellow ground-growth flower'). This overlapping does not seem to be a case of ambiguity about category boundaries, but simply an acceptance of cross referencing of taxa, depending on which feature of a plant is being stressed. I found a similar overlapping of general categories in Haida (an unrelated language), where plants were referable to either a "leaf/medicine" category or a "flowers" category depending on context, and on which aspect of a plant was being stressed (Turner 1974:36).

The "flowers" category, like the "ground-growth" class in each language, includes as primary members plants which were little used for food or as a source of materials. This is not to say that the blossoms of important food species such as spring beauty, yellow avalanche lily or chocolate lily were not called "flowers," but perceptually, they are not within the core of species which comprise this class. They are not normally given as examples of "flowers." There is, at least in Thompson, a utilitarian aspect of many, but not all, members of the "flowers" category that is apparent only on close scrutiny. Various types of flowers were used, more than members of any other class of plants, as charms, to bring luck in love, gambling and the acquisition of wealth. Flowering species such as red columbine, calypso, blue harebell, delphinium, large-leaved avens, bog orchid, alpine bitterroot, red monkeyflower, forget-me-not, night-flowering catchfly, campion and starflower were used for little else, but all were used as charms of some sort (Turner *et al.* 1984: Table 7).

Brown (1984:10) argues that English "flower" is a special-purpose class, " . . . since its membership is not based on several clustering morphological features, but rather

TABLE 7.—*Correspondence of Lillooet and Thompson major plant taxa described in this study with the criteria for life form recognition as defined by Berlin, Breedlove and Raven (1973) and Brown (1984).*

	Life form Criteria*							Qualifies
General plant classes**	1	2	3	3A	4	5	6	
Lillooet:								
"tree"	X	X	X	—	X	X	X	YES
"hay"	X	X	—	E	X	—	—	NO
"grass"	X	X	—	E	X	X	—	NO
"low . . . plants . . ."	X	X	—	E	X	—	—	NO
"flowers"	X	X	—	E	(X)	—	—	NO
"berries/fruits"	X	X	X	—	(X)	—	—	NO
"mosses . . ."	X	X	—	E	X	X	X	NO
"mushrooms and fungi"	?X	X	X	—	X	X	X	YES
"edible roots . . ."	X	—	X	—	(X)	—	—	NO
Thompson:								
"large trees"	X	X	X	—	X	X	X	YES
"tall bushes . . ."	X	X	X	—	X	X	X	YES
"grasses/hay . . ."	X	X	X	—	X	X	X	YES
"low . . . plants . . ."	X	X	X	—	X	—	—	YES
"flowers"	X	X	X	—	(X)	—	—	NO
"berries/fruits"	X	X	X	—	(X)	—	—	NO
"mosses . . ."	X	X	—	E	X	X	X	NO
"mushrooms and fungi"	?X	X	X	—	X	X	X	YES
"large vines"	X	X	—	—	X	X	X	YES (recent)
"edible roots . . ."	X	—	X	—	(X)	—	—	NO

*Criteria for life form recognition are as follows (after Berlin, Breedlove and Raven, 1973:215) and Brown (1984:18-21): **1.** occur at rank Level 1, immediately below unique beginner; **2.** are labelled by primary lexemes; **3.** are polytypic, including many [ten or more] labelled taxa (usually labelled by primary lexemes, although Brown cites examples where all included classes are labeled by secondary, or composite, lexemes); **3A.** are "Empty" (E), including few [under ten] or no named members but many unlabelled taxa. (Such categories are classified as "not full-fledged" life forms, but "incipient" life forms by Brown 1984:21); **4.** reflect natural morphological discontinuities; **5.** are general pur-

pose, not special purpose (ie., are not defined largely by a single utilitarian attribute); **6.** are mutually exclusive (although Brown cites instances where dual life form membership is evident). "X" indicates conformity with life form features. (X) indicates partial conformity. "—" indicates non-conformity. In **3A,** evaluation as an "Empty" category (E) is contrary to the characteristics of a true life form; in this case, a negative assessment (—) implies conformity with life form characteristics.

**See Tables 2 and 3 for native designations and more detailed descriptions of these classes.

simply on the presence of prominent or ornamental blossoms." According to his view, the Lillooet and Thompson "flower" classes would not qualify as "life forms" because they would be attributed the same special-purpose status. However, Randall and Hunn (1984:341) maintain that in both English and Sahaptin "having" a flower is not the only attribute of plants that are "flowers." Most are herbaceous with a showy inflorescence. Part of the confusion is that there is a polysemy between the plant "part" and the plant "class." The distinction is evident in the words of one of my relatives, describing a particular plant to my daughter: "It *has* a flower, but it's *not* a flower." Thus, Randall and Hunn suggest that "flower" should qualify as a life form as much as "grass," "tree" or "vine."

The "berries/fruits" category, as a special purpose utilitarian class, does not fit into Berlin's or Brown's concept of a "life form" at all, but it does exist as a named, psychologically valid general plant class in the various northwestern North American native languages I have studied (cf. Turner 1974:36, 39, 41, 79; Turner and Efrat 1982:21; Turner *et al.* 1984:48). Randall and Hunn (1984:340) have also recognized "edible fruit" as a major inclusive class in Sahaptin. In both Lillooet and Thompson, not only is it recognized with a general class name but additionally, a suffix, ***-úse?*** (lit. 'round-object' in Thompson and ***-usa?*** (lit. 'round-object'; also 'fruit/potato/money'; cf. also ***-us*** 'face/eye') in Lillooet, is applied in the names of many different types of berries, e.g.:

Lillooet

cac?-úsa? (cf. ***s-cicâ?*** 'crow') — blackcap
swəɬkʷa?ú?sa? — saskatoon, red variety
q̓ʷ?x̌-q̓ʷíq̓ʷx̌-usa? 'black (redupl.) fruit' — northern black currant

Thompson

cəqʷ-c̓iqʷ-úse? 'red-berries' — red huckleberry
səxʷ-suxʷ-úse? 'grizzly-bear-berry' — black twinberry
sc̓ôl̓-se? 'sour/tart-berry' — Oregon-grape
si?h-ûs(e?) 'good-fruit' — saskatoon, "good" variety.

In the Fraser River dialect of Lillooet, ***?ūsa?*** is applied specifically to black huckleberry (Fig. 6), although in Pemberton Lillooet, a different term, ***mexāź,*** is used for this species. In Thompson, black huckleberries, though not called "berries" as they are in Fraser River Lillooet, were nevertheless considered "special." Annie York called them "the head of all the fruits . . . an official fruit" and said that if one was given black huckleberries as a gift, he was expected to give a gift in return. In Okanagan-Colville, at least one speaker, Selina Timoyakin, called them the "chief" of the fruits. In another Salish language, Bella Coola, as with Fraser River Lillooet, their name is polysemous with the general name for fruit (Turner 1974:79, 37). This is true also in Sahaptin (E. Hunn, pers. comm. 1986). The berries seem to have the equivalent status among native fruits that bluebunch wheat grass does among grasses in the Interior Salish area.

FIG. 6.—Black huckleberry, called simply *ʔusaʔ* 'berry/round object' in Fraser River Lillooet.

Obviously, the "berries/fruits" category is an overlapping one, especially with the "tall bushes . . . " taxon. It depends upon context, for example, whether one refers to saskatoon as a "bush" or as a "fruit." The majority of "fruits" (ie., "fruit-bearing plants") are, in fact, bushes, but "berries/fruits" could not be said to be a sub-taxon of "bushes" because there are some, such as strawberries and kinnikinnick, that do not fit into the "bush" category.

"Mosses and moss-like plants" is another ubiquitous general plant category in northwestern North American languages, although it is not discussed by Brown (1984) except in a passing reference to "lichens" (p. 14).[14] In Lillooet and Thompson, as with other northwestern North American languages, mosses and lichens are generally classed together (along with liverworts and other moss-like plants such as clubmosses), although differences in color and form are often given recognition. Some rather important or unique lichens, such as black tree lichen, wolf lichen and lung lichen, are called by more specific names, and may not even be perceptually included in the residual "mosses . . . " category by some individuals. In Thompson, lichens, especially the broad, thallose species, are sometimes called ***p̓əp̓éy̓tɬe təɬ qʷzém*** ('frog moss'). Some native speakers consider them to be "a kind of moss," whereas others say they are "similar to mosses."

Habitat and growth form seem to be the overriding factors in distinguishing among various types of mosses and lichens. In Lillooet, for example, various thallose lichens growing on the ground (e.g. *Peltigera aphthosa)* are called ***ƛ̓ən̓ʔ-úlm̓əxʷ*** 'ground/earth-ear'; those growing in trees (e.g., lung lichen) are called ***ƛ̓ən̓ʔ-álqʷ*** ('wood/tree-ear'). In Thompson, mosses and lichens growing on trees are called ***qʷzem-ə̓yqʷ*** ('wood/tree-moss') and those on the ground, ***n-qʷzem-úym̓xʷ*** ('ground-moss'). Additionally, Annie York recognized several other loosely defined categories: "long moss," "short moss," "rock moss," "water moss" and "swamp moss." She, as a native botanical specialist, could distinguish many different types of mosses and lichens within these categories, but many of the people consulted, like most non-native people in the region, did not

perceive many different kinds. As Alec Peters, a Pemberton Lillooet speaker, remarked when shown a variety of different mosses and lichens, "They're all ***páʔsəms*** to me!"

In both languages, the "mushrooms and fungi" category is an important one, including several named species of high cultural significance and a large number of residual unnamed types of little cultural significance. The class names themselves certainly must have derived, or are in the process of deriving, through expansion of reference, from the generic-level names of the most salient types of mushroom. Towards the coast—in Lower Lillooet and Lower Thompson—as well as in the Thompson dialect spoken around Lytton, it is the name for pine mushroom [Lillooet — ***(s)-q̓əm̓s;*** Thompson — ***q̓ám̓es***] that is elevated to the general class name, at least optionally. For the Nicola Valley Thompson, the "cottonwood" mushroom (Fig. 7) has become the "type" for mushrooms in general, and its name, ***məx̌ʷ-qīʔ,*** is used, at least by some, for mushrooms generally, including commercial fresh or canned mushrooms (Mabel Joe, pers. comm., 1985). In the Fraser River dialect of Lillooet, the term ***məlx̌-qín,*** (said to be borrowed from Shuswap; lit. 'cottonwood/stick-head/top') pertains to "cottonwood" mushroom specifically and a cognate form of the Thompson term, ***məx̌ʷ-qiʔ, (s)-məx̌ʷ-áqaʔ*** is applied as a general name for "any mushroom" (e.g. Desmond Peters, Bill Edwards, pers. comm., 1985; cf. also Turner, Kuhnlein and Egger, 1987).[15]

"Vine" is considered to be one of the five "universal life form" categories by Brown (1984). However, there are very few indigenous vine species in northwestern North America. Hence "vine" as a major plant category has low salience in Lillooet and Thompson. One outstanding vine species is white clematis, called ***q̓əc̓usnīnina*** (cf. ***q̓əc̓p*** 'tangled/hard to untie') in Lillooet and ***q̓əc̓-q̓əc̓-usnīn̓us*** ('woven-as-it-grows') in Thompson. Another vine, orange honeysuckle, is sometimes called by the same name in Thompson[16] and could be considered as belonging to the same general class. The less common blue clematis, apparently not known to contemporary Thompson people, was formerly called by the folk specific name, ***q̓əc̓-usnínes e xʔ-úym̓xʷ*** ('high-country woven-

FIG. 7.—"Cottonwood" mushroom, the "type" for mushrooms in general in Nicola Valley Thompson.

as-it-grows') in Thompson (Steedman 1930:459). These three species, therefore, would have been the only members of an original "vine" class in Thompson; it could hardly be called a major life form category. Now, however, with the introduction of various cultivated vines, such as grape, Virginia creeper and English ivy, the "vine" class has expanded, and the name ***q̓əc̓-q̓əc̓-usnīn̓us*** now has, to some people but not to others, broader application as a general term for "vine" in Thompson. In Lillooet, however, there is no real evidence to indicate the existence of a "vine" class, past or present.[17]

Randall and Hunn (1984:340) report that in Sahaptin there is a named, highly inclusive "edible root" class. In Lillooet and Thompson "edible roots [and other underground parts]" were also of high importance in the traditional economy, but in neither language is there a term which refers to any and all edible underground parts, or to plants which bear them. Nevertheless, plants such as spring beauty, avalanche lily, bitterroot, tiger lily, chocolate lily, and wild onions do seem to form a discrete perceptual unit. Most Lillooet and Thompson speakers discuss them as a group, one after the other. For example, Annie York, in talking about combinations of foods, noted: " . . . The food that goes with meat is avalanche lily[18] corms, bitterroot, tiger lily bulbs, chocolate lily bulbs, spring beauty corms . . . and if you can get balsamroot . . . you eat it with meat, and that wapato . . . and the other kind, "wild carrot" . . . they don't mix that with anything . . . and the silverweed . . . they just cook that by itself . . . " Within the course of the conversation she had mentioned, consecutively, nine "root" foods. If the languages had continued to develop without European influence, I would speculate that names would evolve for the incipient "roots" categories in Lillooet and Thompson, probably, as happened for Nitinaht (Turner *et al.* 1983:48), by a process of expansion of reference of the name of one of the most important types of edible roots, perhaps bitterroot or yellow avalanche lily.

Some Lillooet and Thompson taxa do not fit within any of the major "life form" level categories mentioned here. Fireweed, for example, seems to fall conceptually between the "tall bushes" and "low herbaceous plants" categories, but not actually within either. Cow-parsnip, too, is not generally incorporated within any of these major classes, nor are the large fern species, although Annie York used the term ***stuyt-ūym̓xʷ*** for two small ferns, parsley fern and licorice fern.

Other plant classes in Lillooet and Thompson could well be considered that are comparable to some of those already described. These include "medicines" and "water plants." These classes are less rigorously defined and overlap broadly with other major categories. Thus, their relationship with other major plant classes is not hierarchical. I will discuss them in a future paper along with a whole range of other heterogenous "intermediate" level plant classes existing in these two languages.

DISCUSSION

Lillooet and Thompson conform with many other pre-industrial language groups in having no single free-standing term denoting "plant," but they and a number of other northwestern Salishan and Wakashan languages do incorporate "plant" suffixes in many of their generic-level plant names. This phenomenon is somewhat comparable to the use of "plant" in "eggplant" or "spiderplant" but is more pervasive. Many generic level names are developed by adding the "plant" suffix to the term for the most salient part of the plant. This would be as if, in English, the use of "potato *plant*," "strawberry *plant*," "globe artichoke *plant*" were more or less mandatory when referring to these plants in an abstract context.

The origin of this suffix is still subject to speculation, but because it is so widespread in the Salishan and Wakashan languages, it must have great antiquity. Possibly it derived from a free-standing word for "tree" or "tree/bush," whose meaning expanded

to "plant, general" but whose form was reduced to a suffix. A similar situation exists with body part terms (C. Brown, pers. comm., 1985). It would be interesting to know how many other languages of pre-industrial societies have concise linguistic means of recognizing "plantness" without actually having in evidence a free-standing term for "plant."

Only the "tree" category in each language and the Thompson "bush . . . " category conform with virtually all the criteria of "life form" classes as prescribed by Berlin and his colleagues (cf. Berlin, Breedlove and Raven 1974:26; Brown 1984:4). The other general categories differ in various ways (see Table 7). However, the "tree" and possibly Thompson "bush . . . " class names appear not to have originated by the most common means (Berlin 1972:71), through expansion of reference from generic-level terms.

Several other general categories in Lillooet and Thompson differ from the Berlin "life form" by incorporating very few named generic level taxa, while at the same time encompassing a large number of recognizably distinct but unnamed terminal taxa. In all cases the named plants within these categories are highly salient, being very common or distinctive and/or culturally significant, whereas the unnamed plants are generally low in cultural importance. In Thompson, for example, there are only about ten named kinds of plants that fit into the ***s-yîqm*** ("grasses") category, yet ***s-yîqm*** could be applied by Thompson speakers to at least 25 kinds of plants that can be distinguished by native botanical specialists. A similar situation exists for the "low, herbaceous broad-leaved plant," "flower," "moss . . . " and "mushroom" categories in both languages. I have termed these "empty" categories because they include few or no named subtaxa while incorporating a large number of covert types. These "empty" categories also exist in Haida, Bella Coola, Nitinaht and other northwestern North American languages (Turner 1974:35; Turner *et al.* 1983:47). They also occur in Sahaptin, but, in this language, the "grass" and "flower" categories exclude the more salient, named kinds altogether, and incorporate only unnamed types of no cultural significance (Hunn 1982:834).

Lillooet and Thompson general categories are not all defined solely by morphological features. As Hunn (1982) observes, there is a utilitarian factor in plant classification which is not accounted for in the scheme of Berlin, Breedlove and Raven (1973:215, 1974:26), although Brown (1984:10) gives some recognition to categories defined by cultural importance in describing "life form" classes. The "berries/fruits" categories in Lillooet and Thompson are the most obvious examples of utilitarian major plant classes. However, the "low, herbaceous broad-leaved plants" taxa also reflect cultural significance. In this case, plants are generally incorporated on the basis of non-use. The utilitarian factor is also evident in the "grasses" category, where a nomenclatural and perceptual distinction is made in Lillooet between "grass" and "hay." Even many members of the "flowers" category in Thompson are attributed characteristics which are not solely morphological—namely some magical power that makes them good for charms.

As these examples show, there is sometimes no clearcut distinction between morphological and utilitarian features of plants. The essence of "treeness" is the woody xylem tissues (and to a lesser extent, the phloem tissues of the bark) that support the tree and allow it to attain its large size. The woody tissue, because of its chemical composition and physical attributes, also makes a good fuel and an ideal construction material. It is not surprising, therefore, that "wood" and "tree" are synonymous in many languages, or that the name for "tree" often derives historically from the name for "wood" (Witkowski, Brown and Chase 1981:3). Another example of the dualism of certain taxa is seen in the Haida term ***xil,*** which means both 'leaf' and 'medicine' and is the name of a major class of leafy, herbaceous plants, even being incorporated mandatorily in the generic-level names of many members of this class (Turner 1974:31). Van Eijk (pers. comm. 1986) points out that the Salish general plant suffix itself has a utilitarian bias because it most often applies to plants having culturally significant products.

Finally, whereas the Berlin and Brown "life form" classes are mutually exclusive (cf. Berlin, Breedlove and Raven 1974:25), I have found, in conversations with native speakers of Thompson, Lillooet and other northwestern languages, that the structure of taxonomic categories for plants is not rigid, and that, depending on the context of conversation, a particular plant could be assigned to two or even three different major classes. Just as in English our class of "wild flowers" might sometimes incorporate wild rose, and at other times wild rose would be called a "bush," so in Thompson, wild rose might be included as a "bush," a "flower" or a "fruit," depending on context. In English folk classification, "vine maple" might be termed under various circumstances a "shrub" or a "tree" (but not usually a "vine"), so it might in Lillooet or Thompson be assigned to the "bush" or "tree" class. The overlap between "flowers" and "ground-growth" has already been demonstrated. There is, of course, individual variation too, as to which plants are perceived as belonging to a particular class, just as in English, different people may have different ideas about which plants are classed as "weeds."

There is no way of knowing, without further study of all the Salish languages and a reconstruction of proto-Salish, whether Lillooet and Thompson major plant categories follow the lexical encoding sequence for folk botanical life-forms proposed by Brown (1984:24). Brown (1984:143) classes Lillooet as a "Stage 3" language, having only two life-forms, "tree" and "grass," with an incipient "grerb" class, based on his interpretation of information in Turner (1974). Thompson was not included in Brown's survey. If it were, it would qualify as a "Stage 6" language, since it has named classes for "tree," "grass," "grerb," "bush" and "vine." It seems strange that two closely related languages, which must surely have evolved within the same time frame, should be shown by this scheme to be so different. Perhaps if other major classes were included, such as "mosses . . ." and "mushrooms," which are probably more relevant to peoples of northwestern North America than "vines," there would not be such a discrepancy. Additionally, the differences would diminish if the Lillooet "grerb" class were given real status, equivalent in its scope and application to "tree," even though it has few or no named members.

SUMMARY AND CONCLUSIONS

Lillooet and Thompson, two closely related Salish languages of northwestern North America, are similar, though not identical, in their perception and encoding of major plant classes. Both lack free form terms for "plant," but have suffixes on many of their generic-level plant names which denote "plantness." Both have named categories of "trees," "grasses," "low, herbaceous, broad-leaved plants," "mosses," "mushrooms" and "berries." Thompson also has a named category of "bushes," whereas this class is only incipient in Lillooet. In both languages there is an incipient, but unnamed class of "edible roots."

The historical derivation of the category names is still subject to question. The term for "trees" may well have originated in a manner contrary to the most common derivation pattern suggested by Berlin (1972:71), as may have the Thompson name for "bushes." Names for "grasses," and/or "hay" on the other hand, probably originated through expansion of reference of the generic-level name for bluebunch wheat grass.

A number of the major categories are at least partially defined by utilitarian, rather than solely morphological features. These categories are not necessarily mutually exclusive. Most are residual, having a few highly salient named terminal taxa and many recognizably distinct, but unnamed, members. Most of the named taxa have, or had in the past, a high level of cultural significance, particularly as foods, materials or medicines.

The major premise of folk classification systems appears to be convenience to the originator and user. There are no rules that must be adhered to, as developed in our modern scientific biological classification system. The famous linguist Edward Sapir once wrote:

"Unfortunately, or luckily, no language is tyrannically consistent. All grammars leak." (Sapir 1921:38). One might equally well say, "All folk taxonomies leak." The major criterion in folk systems is that they allow communication between members of a society. It is not surprising, therefore, that folk categories like those of Lillooet, Thompson or English are fluid and sometimes ambiguous. Context is of paramount importance. Intonation, gestures and the way words are used within a general topic of conversation, can convey as much as the actual words themselves. These can never adequately be incorporated into such a brief description by a non-speaker of a language. For almost any rule one tries to formulate concerning the attributes of terminology, whether it be the hierarchical nature of folk taxa or their historical encoding, there will be exceptions, probably many. Lillooet and Thompson major plant categories are good examples.

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NOTES

[1]In later papers I will discuss less inclusive plant categories in Lillooet and Thompson—those at the "intermediate" and "generic" ranks.

[2]There is still some question among scientists whether fungi should be considered plants, or whether they are different enough from other botanical organisms that they should be considered as belonging to a separate "kingdom." Even Algae are now frequently classed in their own "kingdom." Blue-green algae and related bacteria are generally classed in a separate "kingdom". Slime molds, once considered close to fungi, are now usually included in a "kingdom" with simple, often one-celled organisms (cf. Whittaker 1969).

[3]Scientific names are provided in Appendix 2. Throughout this paper, native terms cited are written in the orthographies of the original sources, except in Lillooet, where symbols and forms are from Van Eijk (1985). In Thompson, the orthography is standardized after L. C. and M. T. Thompson. However, note that here, *x̣* is written as *x̌* throughout, and, for simplicity, not all of the Thomp-

son's symbols indicating underlying analyses of Thompson terms are shown. These may be seen in Turner *et al.* (1984).

[4]In this paper, in reference to translations of native terms, single quotation marks are used for literal translations, and double quotation marks for general English glosses or interpretations of application.

[5]Examples of similar suffixes in neighbouring Salish and Wakashan languages include (transcription as in original sources; note ***ɬ*** = ***lh*** = ***λ***): Shuswap — ***-álhp/-eλp*** (Palmer 1975:65; Kuipers 1974:64); Okanagan-Colville — ***-ilhp*** (Turner, Bouchard and Kennedy 1981:120); Columbian — ***-alp*** (D. Kinkade, pers. comm. 1986); Halkomelem — ***-elhp/-lhp*** (Galloway 1982:5); Straits Salish — ***-ič̓*** (Turner and Bell, 1971:87); Squamish — ***-ay/-ay̓*** (R. Bouchard, pers. comm. 1976); Sechelt — ***-ay*** (J. Timmers, pers. comm. 1972); Comox — ***-ay*** (R. Bouchard, pers. comm. 1973); Upper Chehalis — ***-inɬ/-inɬ*** (D. Kinkade, pers. comm. 1986); Bella Coola — ***-lhp*** (Turner 1973:209); Nitinaht — ***-ápt*** (Turner *et al.* 1983:90); Hesquiat Nootka — ***-mapt*** (Turner and Efrat 1982:68); Kwakwala — ***-m̓es/-7ems*** (Turner and Bell 1973:288). (The last three languages are Wakashan, a language family distantly related to the Salish Family; hence the suffixes are possibly cognate with the Salishan suffixes.)

[6]Kinkade (pers. comm. 1986) cites further evidence for an original meaning of 'upright object' (singular) rather than 'tree': in both Thompson and Columbian, the plural form for 'tree' has a completely different stem than the singular, and in Columbian the plural means 'bushes, brush' as well as 'plural upright objects.'

[7]Van Eijk (pers. comm. 1986) notes that some Lillooet speakers identify ***c̓q̓áq̓ɬəp*** with ***q^{w}əla-qin/q^{w}əlaqɔ̓qən*** "young tree, sapling," whereas one of the most knowledgeable speakers describes the former as an older, but still not mature, growth stage and the latter as a younger stage.

[8]Randall and Hunn (1984:333) describe such a situation in the Sinama language, where originally there was a "bird-and-moth" class, which has come to conform with folk English "bird."

[9]Hill-tout (1905:213) defines these terms as "long" and "short" grass respectively. Perhaps this reflects their original dichotomy.

[10]For some Lillooet and Thompson speakers the so-called "swamp grasses" are not really "grasses" at all. These "non-grasses" include a number of *Carex* species, one *Scirpus* species *(S. microcarpus)*, both in the sedge family, and even some true grasses that grow in standing water. Other speakers, however, include these as a subcategory of "grasses."

[11]Here is a case of overlapping of general categories, since "speargrass," cited as an example of ***s-wa?p-úlm̓ x^{w}*** "weed," is also classed as ***s-c̓epeẓ̓,*** "grass."

[12]Webster's Dictionary defines "weed" as "any useless troublesome plant," although its derivation from Old English ***weod,*** "an herb," is indicative that it did not always have such a negative connotation. Many English folk names for plants not necessarily "useless" or "troublesome" (e.g., fireweed, gumweed) incorporate the term.

[13]One of the Nicola Valley Thompson speakers recalled that there was also a special meaning for the term, ***stuyt-úym̓x^{w}***, more specific than the general category name. She said, "I heard a lot of another [plant], they call that ***stuyt-úym̓x^{w}*** too . . . high up in the mountains, you have to find a special plant, ask the plant for good luck. See, there's a male and a female plant, and you name that plant . . . the woman's name and the man's name [if you want to use the plant as a love charm] . . . and you put them together and you tell them to live together . . . That's what they call ***stuyt-úym̓x^{w}***, I think." She went on to say that it was a plant that one might dream about, that would give you luck or success (Mabel Joe, pers. comm., 1984).

[14]Hunn (pers. comm. 1986) is inclined to categorize "moss" as a polytypic generic taxon (like Tzeltal "butterfly"). It is true that most, if not all, of the labelled taxa encompassed by this class have names that are secondary, or composite, lexemes. However, the native speakers with whom I have discussed this class seem to accord it the same status as "tree" or "grass," and the specialists among them recognize many different types, even if not all are named. Hence I have included it here as a general class. If it is a generic, it is perceptually at rank level 1 in Berlin, Breedlove and Raven's (1973) folk taxonomic hierarchy.

[15]There is a general nomenclatural and perceptual division in Lillooet and Thompson reflecting the natural discontinuity between the soft, fleshy "mushrooms" growing on the ground, and the tough, leathery shelf, or bracket fungi, mostly in Polyporaceae, growing on trees and stumps. The latter are all called ***q̓əm̓s-ălq***^{w} ('wood/tree mushroom') in Lillooet and ***skel-ule -ĕyq***^{w} ('great-horned-owl-wood/tree') in Thompson. It is debatable whether these bracket fungi are considered to be "kinds of" mushrooms, or only "like mushrooms."

[16]Orange honeysuckle is also sometimes called ***(n)-tɬ̓/tl̓-ûym̓x***^{w} (lit. 'trailing-over-the-ground'), a term also sometimes applied to twinflower, and to other low, creeping plants such as *Lathyrus* and *Vicia* species, both indigenous and introduced. This term could thus constitute another general category name, but is more likely an intermediate-level class included within the general ***stuyt-ûym̓x***^{w} category in Thompson.

[17]Hunn (pers. comm. 1986) notes that in Sahaptin there is only a generic term ***tam-qiks-kula*** for the white clematis; apparently neither orange honeysuckle nor blue clematis is recognized.

[18]Annie York used the Thompson names for these plants in her conversation.

APPENDIX 1. Native language speakers consulted in this study (in alphabetical order of their last names, with year(s) interviewed)

Lillooet

Bill Edwards, Pavilion - 1985
Martina LaRochelle (late), Lillooet - 1972
Margaret Lester, Mount Currie - 1984, 1985
Charlie Mack, Mount Currie - 1974, 1985
Sam Mitchell, Fountain (late) - 1972, 1973, 1974
Edith O'Donaghey, Lillooet (originally Shalalth) - 1985
Alec Peters, Mount Currie - 1984, 1985
Desmond Peters, Pavilion (originally Shalalth) - 1985
Baptiste Ritchie (late), Mount Currie - 1974
Nellie Wallace, Mount Currie - 1984, 1985

Thompson

Lizzie Aljam, Coldwater - 1984
Mary Anderson, Fourteen-Mile - 1980
Bernadette Antoine, Coldwater - 1984, 1985
Hilda Austin, Lytton - 1981, 1982
Janet Charters, Nooaitch - 1984
Nora Jimmie, Nooaitch - 1984
Mabel Joe, Shulus - 1984, 1985
Julia Kilroy ("Shuli"), Coldwater (late) - 1984, 1985
Louie Phillips, Lytton - 1974, 1981
Annie York, Spuzzum - 1973-1985

APPENDIX 2. Scientific names of plant species mentioned in this paper (in alphabetical order of English common names)

alder, mountain *(Alnus crispa)*
alfalfa *(Medicago sativa)*
anemone, western *(Pulsatilla occidentalis)*
arnica *(Arnica* spp.)
avalanche lily, yellow *(Erythronium grandiflorum)*
avens, large-leaved *(Geum macrophyllum)*
balsamroot *(Balsamorhiza sagittata)*
bitterroot *(Lewisia rediviva)*
bitterroot, alpine *(Lewisia columbiana)*
blackcap *(Rubus leucodermis)*
bleeding-heart, wild *(Dicentra formosa)*
bluebunch wheat grass *(Agropyron spicatum)*
bracken fern *(Pteridium aquilinum)*

"bunchgrass" (see bluebunch wheat grass)
buttercup *(Ranunculus* spp.)
calypso *(Calypso bulbosa)*
campion *(Silene* sp.)
catchfly, night-flowering *(Silene noctiflora)*
cedar, western red *(Thuja plicata)*
cherry, bitter *(Prunus emarginata)*
chocolate lily *(Fritillaria lanceolata)*
clematis, blue *(Clematis columbiana)*
clematis, white *(Clematis ligusticifolia)*
columbine, red *(Aquilegia formosa)*
cottonwood, black *(Populus balsamifera* ssp. *trichocarpa)*
"cottonwood mushroom" *(Tricholoma populinum)*
cow-parsnip *(Heracleum lanatum)*
cranberry, highbush *(Viburnum edule)*
currant, northern black *(Ribes hudsonianum)*
"cut-grass" *(Scirpus microcarpus)*
delphinium *(Delphinium* spp.)
Douglas-fir *(Pseudotsuga menziesii)*
fir, grand *(Abies grandis)*
fir, subalpine *(Abies lasiocarpa)*
fireweed *(Epilobium angustifolium)*
forget-me-not *(Myosotis laxa)*
fungi, bracket or shelf *(Polyporus* spp., *Fomes* spp., *Ganoderma* spp.)
goldenrod, Canada *(Solidago canadensis)*
harebell, blue *(Campanula rotundifolia)*
hawthorn, black *(Crataegus douglasii)*
hemlock, western *(Tsuga heterophylla)*
honeysuckle, orange *(Lonicera ciliosa)*
huckleberry, black *(Vaccinium membranaceum)*
huckleberry, red *(Vaccinium parvifolium)*
Indian hellebore *(Veratrum viride)*
Indian hemp *(Apocynum cannabinum)*
Indian paintbrush *(Castilleja* spp.)
juniper, common *(Juniperus communis)*
juniper, Rocky Mountain *(Juniperus scopulorum)*
lamb's quarters *(Chenopodium album)*
lichen, black tree *(Bryoria fremontii)*
lichen, lung *(Loberia pulmonaria)*
lichen, wolf *(Letharia vulpina)*
licorice fern *(Polypodium glycyrrhiza)*
maple, Rocky Mountain *(Acer glabrum)*
maple, vine *(Acer circinatum)*
monkeyflower, red *(Mimulus lewisii)*
mullein *(Verbascum thapsus)*
oceanspray *(Holodiscus discolor)*
onion, nodding wild *(Allium cernuum)*
orchid, bog *(Habenaria dilatata)*
orchid, rein *(Habenaria stricta)*
Oregon-grape *(Mahonia nervosa)*
oyster mushroom *(Pleurotus ostreatus)*
penstemon, shrubby *(Penstemon fruticosus)*
pine, lodgepole *(Pinus contorta)*
pine, ponderosa *(Pinus ponderosa)*
pine, whitebark *(Pinus albicaulis)*
pine grass *(Calamagrostis rubescens)*
pine mushroom *(Tricholoma magnivelare;* syn. *Armillaria ponderosa)*
plantains *(Plantago* spp.)
puffballs *(Lycoperdon* spp.)
pussytoes *(Antennaria* spp.)
rabbitbrush *(Chrysothamnus nauseosus)*
rattlesnake plantain *(Goodyera oblongifolia)*
red-top grass *(Agrostis alba)*
reed canary grass *(Phalaris arundinacea)*
rose, wild *(Rosa* spp.)
rhododendron, white-flowered *(Rhododendron albiflorum)*
rye grass, giant wild *(Elymus cinereus)*
sagebrush, big *(Artemisia tridentata)*
salal *(Gaultheria shallon)*
saskatoon berry *(Amelanchier alnifolia)*
self-heal *(Prunella vulgaris)*
shaggy mane mushroom *(Coprinus comatus)*
silverberry *(Elaeagnus commutata)*
silverweed *(Potentilla anserina* ssp. *pacifica)*
soapberry *(Shepherdia canadensis)*
"speargrass" *(Hordeum jubatum)*
spring beauty *(Claytonia lanceolata)*
spruce, Sitka *(Picea sitchensis)*
starflower *(Trientalis latifolia)*
stonecrops *(Sedum* spp.)
strawberry, wild *(Fragaria* spp.)
sweet-clover, white *(Melilotus alba)*
sweet-clover, yellow *(Melilotus officinalis)*
thimbleberry *(Rubus parviflorus)*
tiger lily *(Lilium columbianum)*
"timbergrass" *(Calamagrostis rubescens)*
timothy grass *(Phleum pratense)*
tobacco, wild *(Nicotiana attenuata)*
twinberry, black *(Lonicera involucrata)*
twinflower *(Linnaea borealis)*
vetch *(Vicia* spp.)
violet, wild blue *(Viola adunca)*
water-parsnip *(Sium suave)*
"wild carrot" *(Lomatium macrocarpum)*
willows *(Salix* spp.)
wormwood, pasture *(Artemisia frigida)*
yew, western *(Taxus brevifolia)*

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HOPLODACTYLUS DELCOURTI (REPTILIA:GEKKONIDAE) AND THE *KAWEKAWEAU* OF MAORI FOLKLORE

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ABSTRACT.—The recent discovery of a giant gecko of unknown provenance in the collections of the Musée d'Histoire Naturelle de Marseille necessitated the employment of relatively unorthodox sources in order to attempt to establish its identity. A combination of morphological characteristics and anecdotal information from folkloric sources assisted in establishing the potential geographic locality of the new species. This determination indicated that an animal heretofore thought to be purely mythical, the ***kawekaweau*** of Maori folklore, is indeed a recognizable biological species.

INTRODUCTION

A hitherto unknown species of giant gecko was recently described (Bauer and Russell 1986) from a specimen rediscovered among the collections of the Musée d'Histoire Naturelle de Marseille, France. It has been in the collections for at least 117 years (Bauer and Russell 1986). The specimen has been assigned to the genus *Hoplodactylus*, a taxon of live-bearing geckos distributed throughout New Zealand but known from nowhere else. Strong anatomical evidence supports this placement of the new species—*Hoplodactylus delcourti*. Although no locality data are available for the specimen, we believe that anecdotal evidence, much of it dating from the last century, may be used to associate this giant gecko with the ***kawekaweau,*** a large reptile of Maori folklore (Bauer and Russell 1986). This therefore links the specimen with New Zealand. The evidence we present serves as an example of the value of native folkloric sources in certain aspects of zoological alpha systematics.

THE NEW ZEALAND GEKKONID FAUNA

The native New Zealand gekkonid fauna consists of two major lineages—the nocturnal "brown geckos" (genus *Hoplodactylus)* and the diurnal "green geckos" (genus *Naultinus)*. (We consider *Heteropholis* Fischer 1883 to be subsumed within *Naultinus*—see Thomas [1982]). Both genera are members of the tribe Carphodactylini within the subfamily Diplodactylinae, a group restricted in its distribution to Australia, New Zealand

and New Caledonia (Fig. 1). Within this group, the New Zealand geckos have their closest affinities with the New Caledonian genera *Rhacodactylus*, *Bavayia* and *Eurydactylodes* (Bauer 1986). In particular, all share a similar arrangement of preanal pores on the venter and a strongly clawed first digit with scansorial pad. Although no uniquely derived characters support the monophyly of *Hoplodactylus* (Bauer 1986), digital characters and scalation allow members of this genus to be distinguished from other carphodactyline geckos.

HOPLODACTYLUS DELCOURTI, ITS AFFINITIES AND PROBABLE PROVENANCE

Hoplodactylus delcourti is distinguished from other members of the genus chiefly by its huge size (370 mm head and body length [SVL]). This makes the giant *Hoplodactylus* by far the largest known gecko species (Bauer and Russell 1986). It is 54% larger than the next largest species *(Rhacodactylus leachianus)* and more than 130% larger than the largest known individual of *Hoplodactylus duvaucelii* (Whitaker 1968), the next largest congener. The significance of and possible origins of such great size are discussed by Russell and Bauer (1986). Whether its point of origin was New Zealand or New Caledonia, *Hoplodactylus delcourti* represents a trend toward island gigantism that has been widely documented in reptiles (Mertens 1934; Carlquist 1974) as well as other vertebrates and invertebrates. New Caledonia, for example, is home to the second and fourth largest extant geckos in the world as well as to one of the largest skinks, a giant grasshopper and a huge flying fox. New Zealand, of course, is noted for the tallest bird, *Dinornis giganteus*, (Cracraft 1976), numerous large skinks and a group of large orthopterans ***(wetas).*** The causes of island gigantism are unclear.

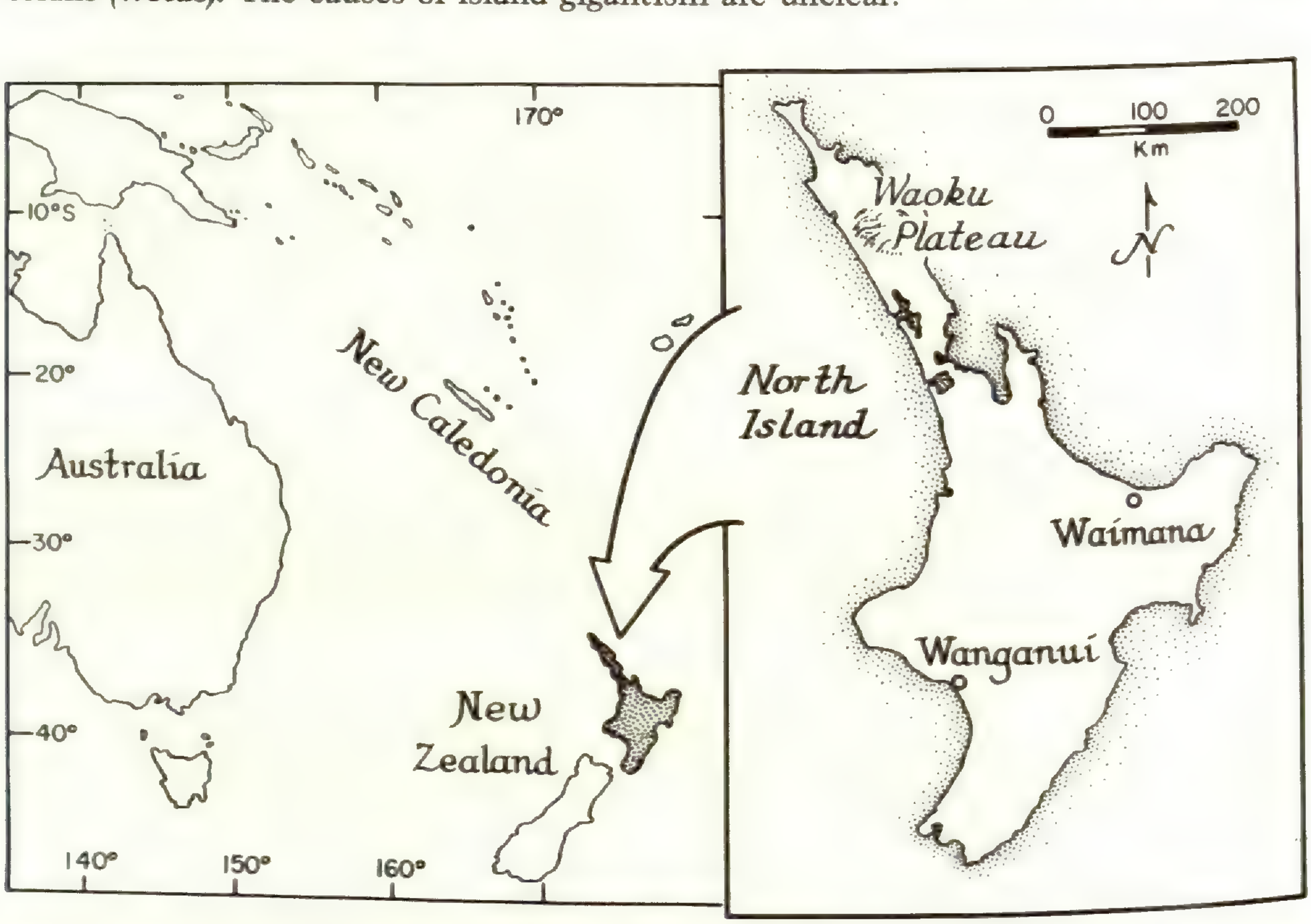

FIG. 1.—Distribution of the tribe Carphodactylini is restricted to Australia, New Zealand and New Caledonia (left map). The ***kawekaweau*** has been reported from Waimana (Mair 1873) and possibly from the Waoku Plateau (Walsh 1905) and Wanganui (Downes 1937), all on the North Island of New Zealand (right map).

Morphological features clearly place the animal in the genus *Hoplodactylus*, but this does not provide information about its origin. The morphology of the specimen limits the areas of possible origin, as it represents a member of a limited radiation of the tribe Carphodactylini in the southwest Pacific Ocean. One possibility is that the animal originated in the French island territory of New Caledonia. A New Caledonian origin would more easily explain the presence of the specimen in a French museum but would require that the genus *Hoplodactylus*, in the recent past, occurred in areas other than New Zealand. An alternative hypothesis, that of a New Zealand origin for *Hoplodactylus delcourti*, would be consistent with current ideas regarding the distribution of this genus, and would not be inconsistent with the history of many areas of the North Island. French explorers, scientists and settlers were quite active in the North Island, particularly in the area of the Bay of Islands (Hocken 1908; Wright 1950). While the museum in Marseille does not possess any other herpetological specimens from New Zealand, it does possess a number of old specimens from Australia and other areas in the Pacific. The dating of the acquisition of the specimen is problematic, but it is most likely that it was acquired between 1833 and 1869. No accessionary documentation of the museum survives from this period. The only New Caledonian material currently in the collection dates from the period 1902-1905 and represents donations from the Marseille Zoological Park, which had received a number of New Caledonian lizards and sea snakes from M. Bernier of the Muséum de Nouméa (Vayssière 1916).

REPTILES AND MAORI FOLKLORE

The contention that the specimen is from New Zealand is also supported by historical and anecdotal records from both Maori and ***pakeha*** (European) sources. Maori knowledge of natural history in New Zealand has been gained in their 900 year occupancy of these islands, but is tempered by common Polynesian conceptions and terminology of nature brought with them from their prior experiences in the islands of the central Pacific (McLintock 1966). There is considerable anecdotal material suggesting the existence of undescribed vertebrates in New Zealand. Of particular note is the case of the "New Zealand otter" (Watson 1960) which has never been resolved. Allusion to unknown reptiles of various sizes and attributes have also been made frequently in the local folklore and lizards, or lizard-like animals feature prominently in Maori art (Best 1923; Skinner 1964. See Fig. 2). Cook (1777) was the first European to record the Maori tales of giant reptiles and many authors have reiterated Maori claims of such beasts (Jameson 1842; Buller 1878; Gisborne 1888; Best 1909). Some of these animals, such as the ***taniwha,*** a giant reptile-like creature, have been summarily dismissed because of the great sizes credited to them. Yet, even in these claims there may be some veracity by way of the correlation of reports of large lizards with crocodiles or monitors (family Varanidae) encountered by the ancestors of the Maoris or collateral groups of Polynesians in other areas of the Pacific (Downes 1937). Lizard-like reptiles of a smaller size also figure prominently in Maori legend. Many of these tales can almost certainly be referred to the ***tuatara*** *(Sphenodon)*, but others seem to describe one or more different animals. The folk taxonomy of the Maori, like that of many peoples, seems to be quite exact (Best 1909), with most of the currently recognized taxa being identified by unique Maori names. Numerous early European workers provided partial listings of Maori names for local animals (e.g. Buller 1878, Downes 1937). Among the herpetofauna of New Zealand, it seems that only the leiopelmatid frogs and some of the more remotely distributed geckos of the South Island were completely unknown to the Maoris. Lumping of biological species also occurred among the skinks, but this is understandable given that modern herpetologists can only distinguish some of the morphologically similar *Leiolopisma* species on the basis of biochemical features (C. Daugherty, pers. comm.).

FIG. 2.—An example of Maori wood carving depicting a lizard-like animal (perhaps a gecko) with ***paua*** (abalone) eyes. This specimen is in the collections of the National Museum of New Zealand in Wellington.

Large lizards known to the Maori were the ***mokomui*** (Kerry-Nichols 1884; Best 1923), the ***kumi*** (Hector 1899) (believed by Hardy [1977] to be mythical), the ***ngarara*** (a 450 mm lizard from Kaiapoi in North Canterbury, Stack 1875) and the ***kaweau*** or ***kawekaweau*** reported from several areas of the North Island. All available reports of these lizards seem to be secondhand, and the characters attributed to the animals are often incongruous. In particular, the reference to the aquatic habits of the ***kawekaweau*** is surely inconsistent with all geckos and most of the known skinks of New Zealand. Indeed, many of the accounts of early settlers may refer to Australian agamid lizards (Hector 1899), such as *Physignathus lesueuri*, a large water-frequenting lizard.

ANECDOTAL SOURCES IN RELATION TO A NEW ZEALAND ORIGIN

A number of the descriptions of the lizards of Maori legend offer corroborating evidence as to the identity and origin of *Hoplodactylus delcourti*. The most valuable information comes from Mair (1873) who commented on "the existence of a large forest lizard, called by the Maoris ***kaweau***". (The spelling and pronunciation of this term varies among Maori tribal dialects—Best 1909.) He continues, "In 1870 an Urewera chief

killed one under the loose bark of a dead ***rata,*** in the Waimana Valley [Fig. 1], he described it to me as being about two feet long and as thick as a man's wrist; color brown, striped longitudinally with dull red". This description, one of the few to provide morphological information about giant lizards, matches extremely well the size and color of the specimen at Marseille, and the position of the lizard under bark is not inconsistent with the known daytime retreats of other large geckos. Buller (1895) stated that the ***kawekaweau*** was semi-arboreal and existed in the deep forests of the North Island at least until the 1860s. He also commented on the striking "banded" pattern of the animal. Best (1909, 1923) referred to the ***kawekaweau*** as being the size of a small ***tuatara,*** reddish in color, and to its retreats as being the holes of trees. This again is a likely gekkonid hiding place and is supported by the known retreats of giant geckos of the related genus *Rhacodactylus* (Meier 1979).

Kerry-Nichols (1884) claimed that a large lizard ***(ngarara*** in the ethnotaxonomic generic sense) was known to local Maoris to inhabit the caves and rock piles of the North Island. Walsh (1905) remarked on an unnamed large lizard, with both arboreal and aquatic habits from the Waoku Plateau (Fig. 1), between the Hokianga and Kaipara districts, the remains of which washed down the Waima Creek around 1870. Finally, Jameson (1842) referred to a large lizard resembling the chameleon. It is noteworthy that in New Caledonia, home of the previously largest known geckos *(Rhacodactylus),* the local name for large geckos is "cameleons", distinguishing them from the small house geckos *(Hemidactylus* and *Lepidodactylus)* which are known as "margouillats". It seems that large geckos are frequently confused with chameleons in areas where chameleons do not occur. This is probably because some species in each family change color, or perhaps because of the prominence of the eyes and relatively slow movements of the animals.

Hardy (1977) suggested that the skink *Leiolopisma gracilicorpus,* might be identifiable as the ***kawekaweau.*** However, the only known specimen of this species has a length of 97 mm and is thus far smaller than the legendary ***kawekaweau.*** Further, the specimen is completely bleached, so that a correlation with the color pattern described in the early accounts is not possible. Given the existence of a specimen matching features of color and morphology as reported by early New Zealanders, we feel that the new species of giant gecko may indeed be the basis for the legendary ***kawekaweau.***

POTENTIAL AGENTS OF EXTINCTION IN HISTORICAL TIMES

Although the lack of material and records would indicate that *Hoplodactylus delcourti* has become extinct, this is by no means certain. There remain in the North Island extensive areas of relatively undisturbed land which might harbor remaining populations. These include areas containing ***kauri*** forests as well as rock outcrops, two of the possible habitats of the ***kawekaweau.*** It is certainly no less likely that this animal still survives than it is that the much larger thylacine *(Thylacinus cynocephalus)* of Tasmania is extant. A number of vertebrate species long thought extinct have been "rediscovered" in the recent past. These include the Chaco peccary *(Catagonus* c.f. *wagneri)* (Wetzel *et al.* 1975), Leadbeater's possum *(Gymnobelideus leadbeateri)* and New Zealand's own ***takahe*** *(Notornis mantelli).* The ***takahe*** (witness the Maori name) as well as at least some species of Moa were present as distinct entities in Maori folk taxonomy. Although most, if not all, the species of Moa had become extinct by the time Europeans had begun to anthropologically document Maori culture, knowledge of the existence of these giant birds had been maintained by oral tradition in at least some tribes (Andrews 1986). Even more noteworthy are recently discovered vertebrates about which nothing was previously known. Two of the smaller, but nonetheless spectacular congeners of *H. delcourti, H. rakiurae* Thomas 1981 and *H. kahutarae* Whitaker 1984 have only recently been discovered in remote parts of New Zealand. It is just possible that a 370 mm SVL gecko

with nocturnal habits, isolated and hidden retreats, and low population density might go unnoticed in New Zealand today.

Whether now extinct or not, *Hoplodactylus delcourti* has not occurred in large numbers for at least a century. Two human factors may be suggested to have contributed to this decline. The first is the degradation of primary habitat through the destruction of native forests. A second is the introduction and spread of introduced mammals by both the Maori and later ***pakeha*** settlers of New Zealand. Stack (1875) suggested rats were responsible for the disappearance of a 450 mm ***ngarara*** from the Waimakariri River Valley, while Buller (1895) cited feral pigs as being the most destructive predators of the ***kawekaweau.*** Taylor (1868) and Kirk (1895) add cats to the list of introduced enemies of reptiles.

Rats have long been regarded as being severely damaging to populations of native reptiles in New Zealand. The effect of rats on the ***tuatara*** has been extensively documented (Crook 1973a, 1973b, 1975; Newman 1983). Although it is currently unclear whether the main factor involved is predation, competition or degradation of habitat, it is known that islands supporting the ***kiore*** *(Rattus exulans)* have fewer juvenile ***tuataras*** and poorer adult recruitment than do similar islands without rats. Whitaker (1973, 1978; Bull and Whitaker 1975) documented that the presence of rats decreases both lizard population size and species diversity on offshore islands. His assumption was that the primary danger posed by rats is predation, especially on young lizards. This view is supported by empirical evidence showing that rats do ingest lizards (Bettesworth 1972; Bettesworth and Anderson 1972; Whitaker 1978). Whitaker (1978) drew conclusions about vulnerability which are pertinent to *Hoplodactylus delcourti:* a) nocturnal species are more susceptible to rats than are diurnal species, and b) larger species are more vulnerable than are smaller ones. In the former case activity time corresponds with peak rat activity, and in the latter animals are unable to use hiding places inaccessible to rats. These conclusions clearly place the ***kawekaweau*** at very high risk.

While no certainty can be attached to the origin of the giant gecko, a body of evidence from historical and ethnographic sources suggests that the species did or does inhabit parts of the North Island. The likelihood of the evolution of such a gigantic form in New Zealand is supported by the preponderance of large species of a variety of taxonomic groups (ratites, scincids, orthopterans, etc.).

ANECDOTAL SOURCES AND THEIR UTILITY IN ALPHA SYSTEMATICS

Common local animals, as well as rarer but more spectacular ones, are strongly associated with the art, folklore and traditions of most cultures. As shown by Maori folklore and legend, as recorded by European chroinclers, the knowledge of native peoples about the fauna may be of value to systematic zoologists. In this particular case the attributes of a relatively rare animal were still known to Maoris who had never seen the animal alive. This is probably due in part to the large size of the ***kawekaweau,*** but also to the fact that mythical powers had been associated with the animal and it therefore took on more symbolic significance. Best (1909:236; 1923:331) stated that it represented the souls of dead forbears of any person who saw it and indicated it was time for that individual to join these ancestors in the spirit world.

Some generalities may be outlined for the use of such anecdotal native evidence in the examination of systematic questions. To the zoologist many native references seem implausible because they attribute biologically incorrect (or impossible) characteristics to animals. In the case of the ***taniwha*** (Downes 1937), virtually all of the attributes of the beast are entirely mythical. The combination of plausible features with mythical ones may, however, imply that the animal in question is based in fact.

Features such as size are particularly likely to be exaggerated. Consequently, reports of animals of relatively small size or rather unspectacular appearance are also more likely to be referrable to living creatures. Unfortunately, rare animals such as the ***kawekaweau,*** had seldom been seen by those describing them. Thus these animals are generally reported as occuring "several villages away" and to have been seen by "a relative" or "acquaintance". Thus again, the fact that some of the Maoris responsible for the claims reported to the ***pakeha,*** whose reports are cited herein, actually saw the animals themselves, near their own villages and in the recent past, adds credibility to the factual basis of the animal in question.

The scientific identification of animals largely from folkloric sources is rare, but such an instance is applicable in the case of *Hoplodactylus delcourti*. In such instances there must always remain a degree of uncertainty of identification. Nonetheless, the reciprocal illumination of the fields of zoology and ethnology holds great potential for uncovering useful information. Greenwell (1985) has reviewed the categorization of cryptozoological evidence and has provided an operational classificatory system for documenting cases. The current case falls somewhere between Greenwell's (1985) categories III and IV. Here we have a taxon represented by a partial specimen and "known" only from anecdotal sources. It is precisely these sources that have aided in its identification.

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The Evolution of Indo-European Nomenclature for Salmonid Fish: The Case of 'Huchen' (*Hucho* spp.). A. Richard Diebold, Jr. Journal of Indo-European Studies, Monograph 5. Washington, D.C.: The Institute for the Study of Man, 1985. Pp. iv, 66. $25.00 (paper).

The salmonid family encompasses salmon, trout, char, huchen, and grayling. In the proposed homeland of speakers of Proto-Indo-European (PIE)—the Eastern Pontic to Western Kirghiz Steppes in the USSR, north of the Black and Caspian Seas—only one salmonid species occurs, ***Salmo trutta,*** with its two morpha, the large anadromous "salmontrout" and the smaller non-anadromous "brook trout." According to Diebold, PIE most likely encoded lexically only two salmonid fish, these being the two types found to the exclusion of all other salmonids in the proposed PIE homeland. This, then, is the evidence supporting the hypothesis placing PIE speakers in the above-mentioned geographical region.

As speakers of Indo-European (IE) languages spread from their homeland through Europe, Western Asia, and the Indian subcontinent, they encountered numerous unfamiliar species of salmonid. Diebold's short book is chiefly concerned with how previously unknown salmonid species were named, and in particular with nomenclature innovated or borrowed for species of the genus ***Hucho*** (huchen).

There are no surprises here. Indo-Europeans have used strategies for naming huchen which are commonly employed by peoples everywhere in labeling newly encountered entities: direct borrowing, description, metaphor, calque, etc. The importance of Diebold's nomenclatural inquiry is that it reveals that *all* terms for huchen in recorded IE languages have been either borrowed or innovated, suggesting that it is unlikely that PIE had a label for any huchen species. According to the author, the same is also true of all IE terms for salmon, char, and grayling, although he does not present detailed documentation for these claims as he does for huchen names. In brief, Diebold makes an excellent but, as he himself cautions, nondefinitive case for a PIE fish lexicon which referentially identified morpha of only one salmonid species, ***Salmo trutta.***

Students of folk biological classification can profit from Diebold's discussion of relatively recent changes in some IE folk taxonomies treating salmonid fish. For example, both huchen ***(Hucho hucho)*** and several species of char (genus ***Salvelinus)*** are lumped together in the Czech folk genus *siven*. Each member of this grouping is binomially labeled: *siven dunajský* ***(Hucho hucho),*** *siven americký* ***(Salvelinus fontinalis),*** *siven alpský* ***(Salvelinus alpinus),*** and so on. Diebold notes that there is an older Czech term for ***Hucho hucho,*** the monomial *hlavatice,* which was used to differentiate lexically huchen from all other salmonids. The inclusion of huchen in *siven* and its binomial designation, then, are relatively recent developments.

Diebold argues that this development in Czech may be related to a "transient Linnaean misclassification" made by 19th century biosystematists. Members of the genera ***Hucho*** and ***Salvelinus*** are perceptually very similar and this no doubt motivated an earlier

mistaken inclusion of these taxa in a single scientific genus. Diebold suggests that this influenced Czech folk classification, wherein both huchen and char developed as members of the folk genus *siven*. He does not speculate on the details of such an influence.

Examples similar to the Czech development have been reported in the literature. For example, the black bear, brown bear, and grizzly bear are included in the folk genus *wu'da*, "bear," in Gosiute, an Amerindian language of Utah (Chamberlin 1908:79). In addition, all three taxa are binomially labeled, e.g., *a-shi wu da*, "grizzly bear." As it happens, there is an alternative monomial label for "grizzly bear" in the language, *oi'ya rro*. I (Brown 1986) have proposed that the latter term for grizzly bear is older and that the development of an additional binomial term for the same is a response to the decreasing salience of grizzlies in the environment of Gosiute speakers. This proposal is in line with my general observation that binomial labels for taxa will replace monomial labels under the condition that the taxa in question decrease in salience or importance for some reason. In the Gosiute case, grizzly bears are no longer salient for speakers of the language since they no longer occur naturally in Utah.

As an alternative to Diebold's suggestion, I propose that huchen have acquired a binomial label in Czech in response to their decreasing salience, e.g., as a result of a decline in the population of huchen in the aquatic environments of Czech speakers. Whatever the case, Diebold has presented us with some important data which are useful in helping to discern regularities in the growth and development of folk biological taxonomies, in addition to informing on such important issues as the identification of the homeland of PIE speakers.

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ZOOARCHAEOLOGY AND TAPHONOMY: A GENERAL CONSIDERATION

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ABSTRACT.—Challenges to increase our understanding of how taphonomic processes potentially affect quantitative and distributional properties of the archaeofaunal record have been responded to by development of an unwieldy terminology and a plethora of control studies on taphonomic processes. A preliminary effort towards integration of concepts and data here involves specification of zooarchaeological analytic goals and a field of study, and alignment of six general taphonomic processes with four general taphonomic effects. The alignment indicates data requisite to taphonomic analyses, and can serve as a reference framework to facilitate construction of a holistic theory of taphonomy from esoteric control study data.

INTRODUCTION

Understanding how taphonomic processes affect quantitative faunal measures is a major challenge facing zooarchaeological research today (Gilbert and Singer 1982; Holtzman 1979; Turner 1983). Quantitative measures such as taxonomic abundances, meat weights, and frequencies of particular skeletal elements are all affected by taphonomic processes (Badgley 1986a; Gifford 1981; Grayson 1979, 1984; Klein and Cruz-Uribe 1984; Lyman 1984a). Not only are quantitative data important in many analyses, but so are the distributions of bones and taxa within a site (Grayson 1983; Lyman 1980; Wheat 1972). Taphonomic processes may obscure distributional contexts; unrelated elements may become spatially associated, or related elements may lose their spatial association (Hill 1979b). The second major challenge in zooarchaeological research is, then, ascertaining the meaning of distributional patterns of bones.

These two challenges have been met in two ways. First, a rather pedantic terminology has been developed in discussions of taphonomy (see Appendix). Many of the words in this unwieldy jargon are only infrequently used by zooarchaeologists and paleontologists involved in taphonomic research. In this paper I avoid this terminology in order to present a general, readily comprehensible and jargon-free discussion of taphonomy.

The second response to the challenges has resulted in a plethora of papers that describe control studies of the decomposition and subsequent dispersal, modification, destruction and burial of one or more animal carcasses and/or their parts (e.g., Abler 1985; Andrews and Cook 1985; Bickart 1984; D'Andrea and Gotthardt 1984; Hill and Behrensmeyer 1984, 1985; Johnson and Haynes 1985; Walters 1984, 1985). Many of these papers discuss some cautionary implications of the control observations. Integration of the data derived from these studies into a coherent whole in paleobiology (Behrensmeyer 1984; Behrensmeyer and Kidwell 1985) and zooarchaeology (e.g., Maltby 1985) has only just begun.

The two responses are rather disparate while the two challenges are interrelated. Here I take steps towards building a general model of taphonomy within which the jargon and control studies might be subsumed to make a more coherent whole. My discussion is specifically directed towards elucidating how quantitative and distributional

characteristics of the faunal record might be conditioned by taphonomic factors. Although I focus on analysis and interpretation of archaeological faunal remains, the discussion is equally applicable to paleontological faunas and more general site formational studies (e.g., Schiffer 1983).

ON THE ANALYSIS OF ARCHAEOFAUNAS

Analyses of archaeological faunal remains have been undertaken at least since the late nineteenth century in North America (Robison 1978). While once scarcely more than a subsidiary endeavor, archaeological site reports now regularly contain a section on recovered faunal remains, often by a specialist, and many more independently published and in-depth studies of faunal remains are being prepared by specialists in zoology and archaeologists with zoological training (Bogan and Robison 1978; Lyman 1979a). This reflects the holistic approach of archaeologists trying to understand and explain the totality of human history.

There are two basic goals to analyzing prehistoric faunal remains: reconstruction of hominid subsistence patterns and reconstructing paleoecological conditions. The former has been characterized as an attempt "to explain, in the form of predictive models, the interface that existed between prehistoric human populations and the faunal section of the biotic community" (Smith 1976:284). This goal is anthropological in orientation as it addresses topics such as human diet, procurement strategies, and predator-prey relationships (e.g., Hildebrandt 1984). Analytic goals are attained using anthropological and ecological principles in analysis and interpretation (Lyman 1982; Rackham 1983). Analyses of paleoecological conditions use zoological and ecological data, methods, and theory (Dodd and Stanton 1981; King and Graham 1981) to reconstruct faunal turnover and succession, paleoenvironmental history, and zoogeographic history (e.g., Grayson 1985).

The two distinguished goals are not mutually exclusive. Both require taxonomic identification of faunal remains, a requirement necessitating adherence to zoological method and theory. Data interpretation requires use of ecological principles whether those concern habitat preferences of taxa or determining available biomass (meat). Interpretation of a single archaeofauna may accomplish either or both goals (King and Graham 1981) because, in part, analytic techniques overlap. Distinction of the two goals is useful to my discussion, but is not mandatory to actual analysis.

BASIC CONCEPTS AND TERMINOLOGY

In the following I focus on animal remains, particularly mammal bones, but hasten to note my remarks are applicable to remains of other animal taxa and plant remains as well. That is, while the field of taphonomic study includes the remains of any organism and its geological context, I restrict my discussion and examples largely to mammal remains.

Taphonomy is generally construed as focusing on the post-mortem, pre- and post-burial histories of faunal remains. Burial is considered to be a stage intermediate to pre- and post-burial histories due to the potentially destructive and disruptive nature of burial processes (e.g., Dixon 1984; Kranz 1974a, 1974b). Various arrangements of taphonomic factors have been posited in the form of models depicting a general taphonomic history (e.g., Gifford 1981; Meadow 1981). Generally, a bone may be buried, exposed, reburied, re-exposed, transported, and reburied prior to recovery. Realistic sequences of taphonomic factors may therefore require the inclusion of loops (Fig. 1). For purposes of this paper, chronologies of taphonomic agents and processes are called *taphonomic histories*.

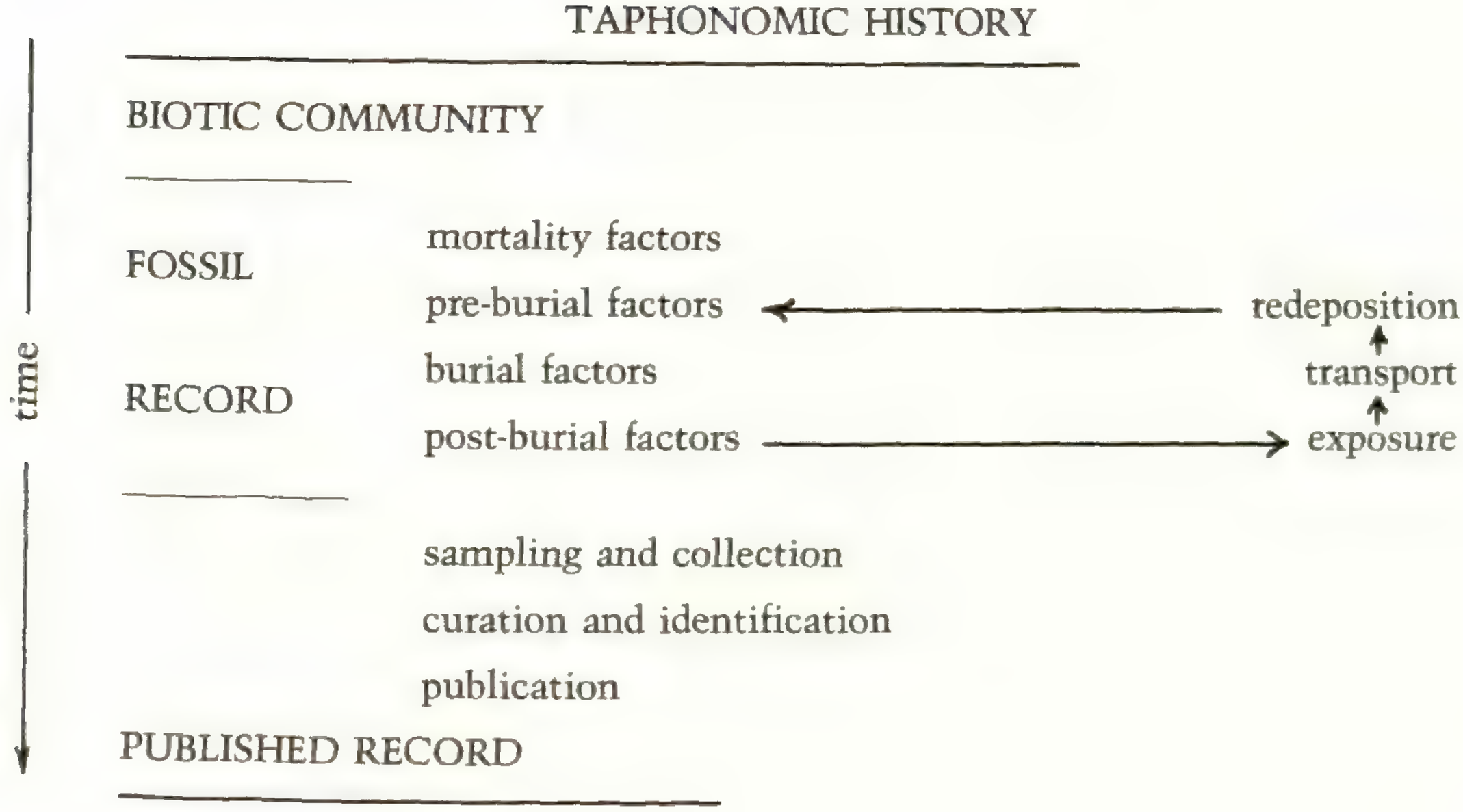

FIG. 1.—Generalized taphonomic history.

A taphonomic history begins when one or more members of a biotic community die. Post-mortem events may include any fraction or the total of any of the processes listed or implied in Fig. 1. It is important to realize that recovery is a potentially biasing factor because it affects the collected assemblage through differentially moving and dispersing it. What the collector perceives as pertinent observations may significantly affect which data are recovered and recorded, consequently affecting final analytic results. A large literature already exists on this crucial topic (Gamble 1978 and references therein). Paleontologists have become much more aware of the stratigraphic and sedimentary contexts of fossils and the potential taphonomic significance of such geological data (Badgley 1986b; Dodson 1980; Krumbein 1965). As a result, more care is taken in the recovery of fossils today than in the past. Archaeologists display equivalent increases in awareness of these issues (Bonnichsen and Sorg 1988).

A *fauna* is some specified set of animal taxa found in a geographic area of some specified size, kind, and location at some specified time (Odum 1971:366-367). For example, one can specify a modern intertidal fauna of the Pacific Rim, a prehistoric terrestrial fauna of Europe, and a Pleistocene mammalian fauna of Colorado. Zoologists study faunas by observing living animals. Paleontologists and archaeological faunal analysts study faunas by analyzing fossils. I have had several other zooarchaeologists tell me "fossils are mineralized animal remains" and/or "fossils are older than 10,000 years." I find neither of these criteria in definitions published by paleontologists (see Appendix). I thus use the term *fossil* here to denote any trace or remain of an animal that died at some time in the past (ascertaining the age of animal death is a separate problem).

A *fossil record* is a set of fossils in some defined geographic space and geologic context. That is, a fossil record consists of those observable phenomena such as the particular bones in a particular stratum. A *fossil fauna* consists of those taxa represented

by the fossil record at a specific locality. The term *fossil fauna* serves to emphasize the taphonomic distinction between a living fauna and a fauna represented by fossils. While the term *fossil fauna* as defined here is virtually synonymous with the term *local fauna* and perhaps *faunule* (Tedford 1970), the first term emanates from the taphonomic perspective of this paper while the latter two terms emanate from a paleoecological perspective.

The fossil record may be in a purely geological context, or in an archaeological context. In the former case, there are no undisputed cultural materials associated with the fossils, while in the latter case there are associated cultural materials. I therefore distinguish two kinds of fossil faunas: those without and those with spatially associated cultural materials, or *paleontological faunas* and *archaeofaunas*, respectively. While the distinction tends to imply whether or not humans had a role in the taphonomic history of a particular fossil assemblage, analytically categorizing a particular fossil record as constituting a naturally or culturally deposited set of faunal remains is a major hurdle (Avery 1984; Potts 1984; Turner 1984). I return to this issue below.

TAPHONOMIC HISTORIES

The paleontological fossil record has been formed totally by natural processes including geological (degradation, aggradation, pedogenesis, etc.) and biological processes (natural senile deaths, predator caused deaths, carnivore attrition, etc.). These natural processes act upon the available organisms (which are in turn conditioned by such natural factors as topography, substrate, vegetation, and climate) and affect the addition to, maintenance in, and subtraction from the paleontological fossil record of organisms and their remains.

The archaeological fossil record is formed not only be the same natural processes as the paleontological fossil record, but also by human processes. An archaeological site, for example, consists of cultural and natural objects that are added, spatially arranged, and preserved and/or destroyed by various human and natural processes. Human processes that affect potential additions to the fossil record include selective hunting (Smith 1979; Wilkinson 1976) and butchery practices (Binford 1978; Noe-Nygaard 1977). Human behaviors that result in the formation of a fossil record have been labeled the "cultural filter" (Daly 1969). Variation in both archaeological and paleontological fossil records is created by varying the additions, the means of addition, and the means of maintenance and subtraction of animal remains from the fossil record.

The distinction between paleontological faunas and archaeofaunas is based on characterizations of their respective taphonomic histories outlined above. It must be emphasized, however, that the characterizations are simplistic. Some fossil records may have no undisputed and spatially associated cultural materials even though humans had an active role in the formation of these records. In the absence of undisputed, associated artifacts, attributes of bone modification attributable to human activities such as butchering and bone tool manufacture are cited as evidence of human intervention. One major example of this involves mastodon (*Mammut* sp.) bones in North America (Fisher 1984a, 1984b; Gilbow 1981; Gustafson *et al.* 1979). In these cases, modifications to bones which are inexplicable given natural processes are cited as evidence of human taphonomic agents even though no artifacts are associated with the faunal remains (see Haynes and Stanford [1984] for similar arguments regarding North American late Pleistocene *Camelops* sp.). However, it is not at all clear as yet whether any of these cases actually represent humanly modified carcasses (e.g., Graham *et al.* 1983).

The literature on attributes of bone modification is expanding rapidly (Bonnichsen and Sorg 1988 and references therein) yet debate abounds over the precise meaning of many attributes (e.g., Johnson 1982, 1985 *versus* Lyman 1984b; and Shipman 1981a; Shipman and Rose 1983 *versus* Eickhoff and Herrmann 1985; Behrensmeyer *et al.* 1986). At present, then, attributes of bone modification used to distinguish archaeofaunas and paleontological faunas are equivocal. Many other kinds of data are necessarily used regularly to help analytically distinguish the two types of fossil records (see TWO EXAMPLES below).

Taphonomic histories are often reconstructed from the abundance and distribution of fossils in a fossil record. For example, in paleontological endeavors a common argument is that the geological context of fossils will provide clues to pertinent taphonomic factors (Behrensmeyer 1975, 1979; Hill and Walker 1972). Features of fossils, such as rounding of fracture edges, abrasion, or surficial modification are also examined (Voorhies 1969). Archaeofaunal analysts have also depended largely on the kinds and distributions of fossils in archaeological sites to infer taphonomic histories (Binford 1978, 1981; Lyman 1985). The objects in a site, their frequencies, spatial loci and associations, and geological and cultural association are all that are observable in the fossil record. A scientific approach to taphonomy must realize what the empirical phenomena of the fossil record are, and produce a model that permits expectations to be phrased concerning fossil assemblage content and distribution; i.e., the archaeologically visible fossil record. Such a model would ideally be universally applicable and yet specific enough to grant insights to particular taphonomic pathways.

A first step to model building involves understanding the basic structure of taphonomic processes and effects. Processes that form the fossil record can be arranged on two dimensions (Fig. 2). The OBJECT dimension consists of addition, subtraction, or maintenance of an object. The SPATIAL dimension consists of movement, and nonmovement. Combining these two dimensions results in six categories of taphonomic processes. All taphonomic processes can be included in this framework whether the taphonomic agent is human or natural. The processes, and their attendant empirical effects, create variability between and within particular fossil records. Because the major expressions of taphonomic effects in archaeological contexts are assumed to be largely attributable to human processes, random variation in fossil assemblage content and distribution is not anticipated, an anticipation borne out by studies of archaeological

OBJECT	SPATIAL: movement	SPATIAL: nonmovement
add	I	II
maintain	III	IV
subtract	V	VI

FIG. 2.—Intersection of two taphonomic dimensions to form six categories of taphonomic processes.

faunas (e.g., Lyman 1978; Meadow 1978; Noe-Nygaard 1977; Pozorski 1979). However, natural processes also create non-random patterns in the archaeological record (Binford 1981, 1983; Brain 1969; Haynes 1980; Hill 1979a, 1979b). The first step in analysis then, is to recognize patterns associated with natural processes. This may be accomplished by first comparing the fossil record under study to random models of the variable(s) under scrutiny, such as skeletal element frequencies or locations. Subsequent to this first comparison, the analyst can begin to assess the meaning of any detected non-random patterns by comparing the fossil record to models of fossil records created by various taphonomic processes. Brain's (1967, 1969) comparison of bone part frequencies in ethnoarchaeological contexts with frequencies of bone parts from South African caves is a classic example of this latter comparative approach.

Taphonomic histories, in general, are initiated when an animal dies. Soft tissues may then be removed, bones may become disarticulated, scattered, buried, fossilized, rot away, and may eventually be recovered by the archaeologist or paleontologist. Of course, various processes may or may not be simultaneous and may or may not affect particular carcasses or bones. The general set of potential effects of taphonomic processes may be arranged into four categories: disarticulation, scattering or dispersal, fossilization, and mechanical modification.

Disarticulation refers to the anatomical disassociation of skeletal elements. Disarticulation is related to soft tissue that functions to hold joints together (Dodson 1973; Hill 1980; Schafer 1972; Toots 1965). Chemical or mechanical breakdown and/or removal of soft tissues ultimately results in disarticulation (e.g., Coe 1978; Micozzi 1986; Payne 1965). Because soft tissue anatomy varies from joint to joint, the process of disarticulation is extremely complex under natural conditions (Hill 1979a), but is not so complex as to preclude construction of models of natural disarticulation (Hill 1979a, 1980; Hill and Behrensmeyer 1984, 1985).

Dispersal or transport of skeletal elements may preceed, or be simultaneous with or subsequent to disarticulation, and is related to disarticulation because it concerns the spatial location of fossils. While disarticulation requires only a few centimeters of spatial disassociation of parts to destroy anatomical integrity, dispersal entails centimeters to kilometers (Hill 1979a). *Dispersal* of skeletal parts means the increase or decrease of distance between bones. Models of dispersal have been constructed for fluvial transport (Behrensmeyer 1975; Boaz and Behrensmeyer 1976; Hanson 1980; Korth 1979; Voorhies 1969), human transport (Binford 1978), raptor transport (Plug 1978), transport by porcupines (Brain 1980), carnivore transport (Binford 1981), and random processes (Hill 1979a).

Fossilization is here meant to denote the alteration of bone chemistry (Cook 1951; Cook *et al.* 1961; Cook and Heizer 1952; Hare 1980; Rolfe and Brett 1969; Schopf 1975; Shipman *et al.* 1984; White and Hannus 1983). Research suggests that the type of sedimentary matrix in which the bone is deposited largely determines the particular types of fossilization processes that bones may undergo. Secondary determinants of fossilization processes include environmental conditions such as soil moisture regimes as determined by precipitation and temperature. Some fossilization processes, especially weathering (Behrensmeyer 1978), may result in fragmentation of bones.

The final readily discernible category of taphonomic effect is *mechanical alteration.* Mechanical alteration denotes the structural and/or morphological alteration of the original living bone by mechanical or physical processes. Common examples of mechanical alteration include fragmentation and abrasion. For instance, each bone in an animal is a complete, discrete object. The cause of the animal's death and/or postmortem factors may result in broken bone (Lyman 1988). Fragmentation, then, is the destruction of original discreteness of a bone by generating multiple discrete objects from the original discrete object by mechanical or physical means, in this case by the physical

loading of force on the bone. Abrasion is the modification of original bone morphology by the application of frictional forces to the bone surface. Models of bone fragmentation are still in developmental stages (Bonnichsen 1979; Evans 1976; Johnson 1985), and abrasional forces are still poorly understood (Behrensmeyer *et al.* 1986; Brain 1967; Morlan 1980; Shipman and Rose 1983).

To date most analyses of fossil assemblages that consider taphonomic issues assess whether or not the fossil assemblage has been transported to its recovery location. Shotwell (1955) was one of the first analysts to develop a sophisticated and objective analytic technique to assess whether or not a fossil assemblage had been transported, and to assess which taxa in the assemblage were locally derived and which were probably intrusive or non-local. While Shotwell's (1955) technique was later adapted to distinguishing naturally from culturally deposited taxa in archaeofaunas (Thomas 1971), it has since been shown to contain serious flaws (Grayson 1978b; Wolff 1973). Analysts still address this issue, using techniques such as assessing the degree of abrasion of bones (Behrensmeyer 1975) to determine whether the assemblage or portions thereof have been fluvially transported to the collection locality, and use of experimental data to allow inferring the agent of transport (Binford 1981; Lyman 1985). Paleoecological interpretation may only be considered realistic when this distinction of transported and nontransported taxa has been made. Inferences regarding human subsistence practices generally consider the transport issue under the larger realm of the "schlepp effect" (Binford 1978; Daly 1969; Lyman 1985; Perkins and Daly 1968) and differential storage of foodstuffs (Binford 1978).

Many ways exist for members of a biotic community to die, and many ways exist for dead organisms to become a fossil assemblage. Different taphonomic histories may result in similar fossil records regardless of the initial biotic community or set of dead organisms (Gifford 1981), the phenomenon of *equifinality*. This is the ultimate analytic challenge of taphonomic research: to determine which of several possible taphonomic histories is actually responsible for a bone assemblage.

GOALS OF TAPHONOMIC ANALYSIS IN ZOOARCHAEOLOGY

Subsistence studies, by the nature of their research questions, require knowledge of the formation of the archaeofaunal record (Lyman 1982; Maltby 1985; Medlock 1975; Rackham 1983). Similar knowledge is important to paleoecological research but for different reasons (Behrensmeyer and Hill 1980; Gifford 1981). Subsistence studies require that the fossils constituting the archaeofauna be sorted into at least two categories: those fossils deposited as a result of human (subsistence and other) behaviors, and those naturally deposited (Binford 1981; Thomas 1971). Culturally deposited fossils must be qualitatively and quantitatively representative of the fauna exploited, and quantification techniques must produce accurate relative abundances of economically important taxa (Grayson 1979, 1984; Lyman 1979b). Paleoecological studies, because of their different goals, need not have representative samples of exploited fauna, but do require representative samples of the prehistorically extant fauna. Exploited and extant faunas need not be similar because human populations may not have randomly exploited extant faunas. Sample requirements are flexible in the sense that they have certain tolerance limits. For example, a bison kill site probably does not include all taxa exploited by a group of people, and a zooarchaeologist may focus only on the microfauna and ignore larger taxa in an archaeofauna, depending on the research questions being asked. Sample representativeness is relative to some population which in turn is dictated by the research goal and is controlled by the formation, recovery and analysis of the fossil record (Fig. 1).

Gifford (1981) distinguishes two basic goals of taphonomic research: (1) "stripping away" the taphonomic overprint from the fossil record to obtain accurate resolution of the prehistoric biotic community, and (2) determining the nature of the taphonomic overprint in order to be able to list the precise taphonomic mechanisms responsible for a given fossil assemblage, enabling the writing of taphonomic histories. The latter goal is analogous to studying formation of the archaeological record (e.g., Schiffer 1983). The former goal is seen as a necessary step towards paleoecological analysis because the target of analysis requires knowledge of the prehistoric biotic community.

Determination of the exact taphonomic history of a particular fossil assemblage is frequently attempted by archaeologists who wish to know which taxa were exploited and the relative proportions in which those taxa were exploited. Many interpretations therefore involve outlines of the suspected human (taphonomic) behaviors that resulted in the fossil record under scrutiny. For example, Wheat's (1972) description of the butchery process evidenced at the Olsen-Chubbuck bison kill site is simply a narrative model of the suspected taphonomic history of that site's fossil record. Other examples include the recent discussions of Plio-Pleistocene sites at Olduvai Gorge (Binford 1981; Bunn 1982; Bunn and Kroll 1986; Potts 1982, 1986) and the Pleistocene site of Zhoukoudian (Binford and Ho 1985; Binford and Stone 1986).

The two goals of taphonomic research are not mutually exclusive. Stripping away the taphonomic overprint requires that the overprint be known. Once the taphonomic overprint is known, the prehistoric biotic community can be determined by analytically reversing the effects of the taphonomic processes. Of course, this procedure requires the assumption that the sample of fossils is representative of the biotic community. This assumption has been analytically controlled in cases where an archaeofauna is directly compared with a paleontological fauna in geographic and temporal proximity to one another (e.g., Briuer 1977), and in cases where two or more geographically and temporally adjacent archaeofaunas are compared (e.g., Grayson 1983; Guilday *et al.* 1978). The covert assumption to such comparative analyses is that because each fossil assemblage has undergone a more or less unique taphonomic history, similar, independent interpretive results derived from the assemblages are thought to represent prehistoric reality. That is, taphonomic processes have not totally obscured all indications of a prehistoric biotic community as all examined fossil assemblages indicate the same community.

A third goal of taphonomic analysis involves explaining the variability in frequencies of *fossil categories*. Any number of fossil categories can be defined, but the two most common ones are taxonomically defined and anatomically defined categories. In the former case, each fossil is identified as representing a particular taxon; in the latter case, each fossil is identified as representing a particular skeletal element. Both categories are important in quantification techniques such as NISP and MNI. The most frequently occurring anatomical category within a taxonomic unit determines the MNI value for that taxon. Variability in taxonomic frequencies is often interpreted as signifying paleoecological conditions (Grayson 1981). Frequencies of taxa measured by counts of anatomical and taxonomic fossil categories are, however, a function of taphonomic processes. Capabilities to explain variability in frequencies of anatomical parts in taphonomic terms will provide understanding of what frequencies of fossil categories are actually measuring, be it paleoecological conditions or something else (Lyman 1984a).

DISCUSSION

Taphonomy is concerned with differences and similarities within the fossil record and the link between a fossil record and the prehistoric fauna from which it derived. Concerning the latter, obvious differences include the presence of living organisms

versus post-mortem remains of organisms. Ecological and ethological studies of extinct taxa are, of course, impossible to do in the detail that living taxa are studied. Even those taxa with modern, living counterparts are not so easily dealt with when represented by fossils because studying living taxa presents certain difficulties (Coe 1980). Differences and similarities between living and fossil faunas present taphonomic challenges to paleoecological research because the ecological principles used to study living faunas are commonly used in paleoecological research (Western 1980; Van Couvering 1980). Consequently, the fossil record must be analytically reconstituted into a fossil fauna or the original biotic community to answer many research questions. Subsistence studies using archaeofaunas face similar analytic challenges (King and Graham 1981; Smith 1979).

Techniques for meeting these analytic challenges can be derived by considering the four categories of taphonomic effects (disarticulation, dispersal, fossilization, mechanical alteration). Each of these effect categories is visible in the fossil record and can be incorporated in an operational model of taphonomy. Comparing taphonomic effects with taphonomic processes (Fig. 2) suggests several points (Table 1). First, process category VI is largely a data recovery factor and not strictly taphonomic. New or additional fossils cannot be added to an assemblage that is in situ without movement of the "new" fossils. The only conceivable way this may happen is if the assemblage moves to a new location and is deposited around the "new" fossils without the latter's movement, as in some fluvial settings (Boaz 1982). A common and readily conceivable way for new fossils to be added to an assemblage without movement of the former is for the sampling universe to be enlarged such that additional fossils are collected.

TABLE 1.—*Comparison of taphonomic processes and taphonomic effects.*

TAPHONOMIC PROCESS CATEGORY	TAPHONOMIC EFFECT CATEGORY
I. move and subtract	A. disarticulation
II. non-move and subtract	B. dispersal
III. move and maintain	C. fossilization
IV. non-move and maintain	D. mechanical alteration
V. move and add	
VI. non-move and add	

PROCESSES ALIGNED WITH PROBABLE EFFECTS
I — A, B, D
II — C
III — A, B, D
IV — C
V —A, B, D
VI — sampling

The second important point deriving from comparison of processes and effects is that different processes can have similar effects; equifinality is a very real problem. Disarticulation, dispersal, and mechanical alteration all involve movement of the fossils while fossilization effects do not require movement. Finally, all effect and process

categories concern frequencies (add, maintain, subtract) and distributions (move, non-move) of fossil categories. It is therefore pertinent to discuss techniques for measuring each taphonomic effect in the fossil record.

Disarticulation seems to correlate with soft tissue anatomy. Hill (1979b:744) for instance, concludes that "the determining controls of the [disarticulation] pattern are inherent in the anatomy of the dead animal itself and thus independent of the agents whereby it is realized." Disarticulation might be modeled by a rank ordering of the cross-sectional area of soft tissue surrounding joints. The basic analytic assumption might be phrased as "the greater the cross-sectional area of soft tissue associated with a joint the longer the joint will remain intact subsequent to the animal's death." This assumption of course presumes that soft tissues associated with each joint are qualitatively identical, which is highly unlikely (Hildebrand 1974; Romer and Parsons 1977). Study of disarticulation in the fossil record requires detailed data on bone location and spatial association.

Dispersal is a complex process minimally controlled by disarticulation, type and strength of dispersal mechanism, substrate, topography, and bone density, size and morphology. Hill (1979a:269-270) begins his discussion of dispersal by hypothesizing that scattering is caused by processes that act randomly. When observed, departures from the random pattern suggest non-randomly acting processes whose identity must be determined. Hill's (1979a) hypothesis could be used as the first null hypothesis to be tested with fossil data. Then, intrinsic properties of bones can be used to generate expectations regarding distributional patterns of fossils (e.g., Frostick and Reid 1983; Korth 1979). Clearly, data on bone location, orientation and angle of dip (e.g., Butzer 1982:100-104) should be recorded during field recovery, as well as sedimentological data indicating mode of deposition and turbation processes (Wood and Johnson 1978).

Fossilization mechanisms are minimally dependent on climate, depositional matrix, and bone porosity. There apparently is no detailed model of fossilization comparable to Hill's (1979b) models of disarticulation and dispersal. Documented processes of fossilization (e.g., Rolfe and Brett 1969; Schopf 1975) indicate, however, that in order to study fossilization, data required include matrix chemistry and mineralogy, chemistry of the fossils and original chemistry of the bones, climatic (past and present) information such as temperature, precipitation, and ground water regimes, and a knowledge of geologic and pedogenic processes forming particular strata.

Mechanical alteration seems to be largely controlled by bone structure and morphology, at both microscopic and macroscopic levels, and bone porosity and density. In order to measure mechanical alteration in the fossil record, the minimal requisite data are frequencies of fragment types (Watson 1979) and whether or not fragments of a bone are associated in situ, polish and abrasion, and other features. For example, Klein and Cruz-Uribe (1984) suggest sediment overburden may crush more deeply buried bones; all else being equal, the analyst could measure fragment sizes to determine if fragments decreased in size with increasing depth. Shipman (1981b:129) also suggests differences in fragment sizes are indicative of differences in taphonomic histories (see Binford [1978] and Schmitt [1986] for ethnoarchaeological and archaeological studies of the taphonomic meaning of differences in fragment sizes, respectively).

Despite the pleas of several authors over a decade ago (Hill 1978; Hill and Walker 1972; Munthe and McLeod 1975), the kinds of data mentioned above are seldom recorded by field crews and/or published by analysts. Because fossil category abundances are important to many traditional analyses and interpretations, frequency data are nearly always published, and more is known about the taphonomy of fossil category frequencies than virtually any other variable of the fossil record. In fact, several recent taphonomic analyses of Plio-Pleistocene sites in Olduvai Gorge, Tanzania, focus

heavily on such frequencies yet fail to adequately consider the range of processes that may have affected those frequencies and also fail to describe data concerning other taphonomically relevant variables (Bunn and Kroll 1986; Potts 1986). The relevance of all variables to complex taphonomic problems is made abundantly clear by considering two examples of taphonomic analysis.

TWO EXAMPLES

I have thus far suggested that taphonomic histories are usually complex, and that taphonomic processes may be arranged in six general categories while taphonomic effects may be arranged in four general categories. Alignment of process and effect categories reveals the types of data required for taphonomic analyses. I also have distinguished two basic goals of zooarchaeological faunal analysis (determination of human subsistence patterns and prehistoric ecological conditions) and two types of fossil records (archaeofaunas and paleontological faunas). All of these issues I have discussed are found in one form or another in taphonomic studies. In this section I review two published taphonomic studies, one I consider to be a good example of careful taphonomic analysis and one I consider to be not so good, to illustrate points raised in preceding sections of this paper.

The not so good.—In the 1960s and 1970s, C. V. Haynes (1969, 1976) argued that indisputable proof of the earliest people in the Americas should consist minimally of unquestionable stone artifacts, multiple and/or demonstrably uncontaminated radiocarbon dates, and a clear, tight, undisturbed stratigraphic association of the artifacts and dates. Stanford (1979) updated these criteria in light of the potential that the earliest Americans may have used non-lithic tools. He suggested that in addition to Haynes' mandatory criteria, archaeologists must learn to recognize bone and antler artifacts, and to distinguish faunal accumulations attributable to human activities from those attributable to non-human agents and processes.

McGuire (1980:263) wished to "document the results of natural faunal activity at a cave site" as part of the larger pursuit of unequivocal indicators of late Pleistocene humans in the Americas. McGuire's arguments rest on the *inference* that deposits at Mineral Hill Cave, Nevada, are natural. As evidence for this inference he suggests that "indicators of aboriginal occupation such as smoke blackening, artifactual material, lithic debris, and burnt bone were not found" (McGuire 1980:264). This is all negative evidence, which would not be so crucial were it not for the fact that only a "2.3% sample was excavated" (McGuire 1982:241). Here lies the first of two basic problems with McGuire's taphonomic analyses.

McGuire sampled 2.3% of the horizontal area of the cave interior. It cannot be ascertained what fraction of the deposit volume was sampled. This plus the fact that artifacts are not generally randomly distributed while McGuire's sample is restricted to one horizontal location makes for a sample that no doubt is not representative of the deposit as a whole. As made quite clear by, for instance, Gamble (1978), archaeologists sample space, not the artifacts or bones that may occur in that space. Hopefully, if appropriate sample techniques are used, a representative sample not only of space will be derived, but a sample of artifacts and faunal remains representative of the complete population of bones and artifacts in the sampled space will be recovered.

Hole (1980) has noted that often a complex sampling technique is chosen with little regard for the purpose (desired data) of sampling (see also Nance 1983). In the case of Mineral Hill Cave, no reasons are given for the sampling design used. Therefore, one

can only wonder why a test pit was not placed near the entrance to Mineral Hill Cave instead of placing both test pits 8 m away from the cave entrance in a dark recess of the cave. Several Great Basin caves which were occupied by prehistoric peoples tend to have the greatest amount of culturally deposited debris in the half of the cave nearest the cave entrance (e.g., Aikens 1970; Jennings 1957). Not only does McGuire's sample appear to be much too small (see also Gruhn and Bryan 1981), but it seems inappropriately located for the purposes of the research. This indicates the importance of sampling and data recovery in taphonomic analyses.

The second major problem is that McGuire (1980, 1982) *infers* that the Mineral Hill Cave deposit is natural. Binford (1981:26-27) notes that "diagnostic signature patterns that discriminate one deposit forming agent or set of agents from another" must be established. "We must see a bear make a footprint to know what a *bear* footprint looks like" (Binford 1981:27). Then, a uniformitarian assumption is made in order to allow identification of animal tracks encountered in the future as having been made by bears or some other animal when the animal itself is not visible. The problem at Mineral Hill Cave is that McGuire did not see the bear, and simply because he did not find any evidence of tracks, he infers the bear was not there. This problem is not McGuire's alone as other archaeologists working with faunal remains have made the same error (e.g., Briuer 1977; Gilbow 1981). Simply comparing a bone assemblage from a site with no artifacts to a bone assemblage from a site with associated artifacts proves little, particularly when the sites are of comparable age and the possibility exists that people were present in the area. This is, in fact, the problem rather than the solution; we simply do not know what a fossil assemblage deposited by people but without associated artifacts should look like. This illustrates that my distinction of archaeofaunas and paleontological faunas, while useful for discussion purposes, is not meant to connote that every bone in the former was deposited by humans while all bones in the latter type of fauna were not deposited by humans.

McGuire (1980) uses several equivocal lines of evidence to infer people were not responsible for the Mineral Hill Cave deposit. First, McGuire (1980:265) argues that the ratio of stone artifacts to bones at two Great Basin cave sites suggests "even an infrequent occupation of Mineral Hill Cave would have resulted in at least some artifactual evidence." We simply do not know enough about how the deposition rates of site sediments, bones, and artifacts may vary and/or covary to make such assertions. The two caves McGuire chose for comparison may or may not be representative of the total range of variation in these rates.

Further, the ratio of stone tools to bones is a fallacious indication of human presence when the concept of expedient bone tools is introduced (Gruhn and Bryan 1981). Expedient bone tools were made from the bones of animals being butchered and were discarded after use with the rest of the faunal debris (Johnson 1982, 1985). Only knowledge of tool production need be brought to a butchering locality. If stone tools were curated, only expedient bone tools may be recovered by the archaeologist. We are far, however, from having established criteria for the identification of expedient bone tools (Lyman 1984b).

The second line of evidence McGuire (1980) employs is the absence of burnt bone. By implication, the presence of burnt bone would signify the presence of people. Balme (1980), however, argues that burnt bone with no associated artifacts deposited in an Australian cave was not deposited by people. The presence or absence of burned bone is at best a highly ambiguous criterion for assessing the presence or absence of people.

The third line of evidence is that the ceiling of the cave was not smoke-blackened. To get smoke-blackening, a fire which produces smoke must be present. Either no fires or smokeless fires were built in Mineral Hill Cave. Alternatively, the history of the cave

subsequent to smokey fires in the cave may have included roof fall events, water seepage over the ceiling, or some other process which effectively removed any evidence of a smoke-blackened ceiling. Clearly, the geological history of the site must be considered in taphonomic analyses.

The fourth line of evidence McGuire (1980:260) refers to is "split long bones of large mammals." Because such broken bones are present in the Mineral Hill Cave deposit, McGuire argues that citation of the presence of such broken bones in sites is an equivocal indicator of human presence. He has, however, failed to (1) convincingly demonstrate humans did not break the Mineral Hill Cave bones (cf. Morlan 1984), and (2) describe the fractured bones from Mineral Hill Cave in such a fashion as to allow comparison with known naturally and culturally broken bones (see Biddick and Tomenchuk [1975] for a clever but seldom used descriptive technique conducive to such comparative studies). This illustrates how critical data are often not recorded and/or reported in many taphonomic studies.

I am unconvinced that prehistoric people never entered Mineral Hill Cave. This is not to say that I believe people did enter the cave, but only that on the basis of the published evidence, it is impossible to determine whether or not people did actually utilize Mineral Hill Cave and contribute to the formation of the deposit. McGuire's sample is inappropriate for the taphonomic question he asks, his analyses fail to consider all relevant comparative data and theory, and he fails to present sufficiently detailed data for other taphonomists to perform comparative analyses. These problems plague many published taphonomic studies; this is not good. Fortunately, not all taphonomic studies are plagued by these problems; some are good.

The good.—Extinction of the large-antlered Irish elk *(Megaloceros giganteus)* has often been explained as resulting from their having become mired down in bog mud and/or being drowned in part because their antlers were so cumbersome. Barnosky's (1985, 1986) analyses of remains of this large cervine are truly examples of taphonomic research at its best.

Barnosky (1985) begins by listing six test implications of the miring-drowning due to large antlers hypothesis. Only one of these is met with available data: all individuals in the bog he sampled were males, as predicted by the model, female Irish elk not carrying antlers. But the other five hypotheses are not met: antlers are smaller than normal on average in Barnosky's sample, skeletons are not articulated nor complete, the bones are embedded in clay deposits too thin for the animals to have become mired down in, the deposits are not disturbed by trampling or struggling of these animals as they should have been by mired down animals, and the bog waters were apparently shallow enough (as inferred from geologic data) to preclude drowning of upright animals. These six test implications required detailed morphometric, contextual, associational, and stratigraphic data, all prerequisites I have noted in previous sections of this paper.

Failing to confirm the miring-drowning hypothesis, Barnosky (1985, 1986) proposes and tests two alternative hypotheses. The Pleistocene overkill by human hunters hypothesis (Martin and Klein 1984) is quickly discarded because no artifacts have been found associated with remains of Irish elk, and "the few examples of modification to Irish elk bones reputed to have been inflicted by humans cannot be distinguished from other naturally created kinds of breaks, abrasions, gnaw-marks, or scratches" (Barnosky 1986:132). Further, the oldest archaeological evidence of humans in Ireland dates between 9000 and 8500 B.P., while the Irish elk was extinct there ca. 10,600 B.P. In contrast to McGuire's (1980, 1982) undated but probable latest Pleistocene Mineral Hill Cave materials, tight chronological and stratigraphic control of Irish elk remains described by Barnosky makes the argument for absence of a human taphonomic agent much more convincing.

The second alternative hypothesis is that "male Irish elk visited bogs more often than females did during winters, when unfit animals died and decomposed near the water's edge, in some cases on the ice, and were scavenged and trampled" (Barnosky 1985:340). Evidence bearing on the six test implications for the miring-drowning hypothesis are consistent with the winterkill hypothesis, as are four other test implications specific to it. All elk died with antlers attached, suggesting an autumn-winter death season. Elk mortality was demographically attritional. Barnosky's sample of Irish elk tended to be small in body and antler size, suggesting "some combination of limited resources, malnutrition, or disease during fetal or postnatal growth" (Barnosky 1985:341). Finally, in modern cervines, "male mortality is greater than female mortality during winter, apparently because males, unlike females, eat little during the fall rut and enter the winter in poor condition," males more often seek winter shelter in valley bottoms (near bogs), and thus may die near lake (bog) shores "because they need water and because they are easy prey on ice" (Barnosky 1985:343), thereby explaining the overabundance of males relative to females in collections of Irish elk fossils. The test implications for the winterkill hypothesis underscore the necessity of age-sex demographic data, morphometric data, and the use of modern analogs as comparative bases in taphonomic analyses.

While Barnosky's (1985, 1986) study deals with paleontological faunas, his careful and detailed analyses should be emulated by those studying archaeofaunas. In fact, Fisher (1984a, 1984b) has made just such an attempt with North American mastodon remains, concluding like Barnosky that winterkill (and malnutrition) was a major mortality factor but that humans exploited (hunted? scavenged?) this large mammal. While not without weaknesses, Fisher's analyses and conclusions warrant careful study.

In summary, Barnosky not only provides many (but not all; e.g., the "scratches" on Irish elk bones are not described) relevant data, he considers three separate hypotheses. The winterkill hypothesis clearly succeeds because its test implications are met and test implications of the other hypotheses are not met. Further, while McGuire (1980, 1982) only considered four test implications, all with negative evidence confirming his hypothesis that people did not use Mineral Hill Cave, Barnosky (1985, 1986) considered a dozen implications, some with negative evidence and some with positive evidence to confirm his hypothesis. All taphonomists should emulate and seek to improve Barnosky's methods: they are good.

CONCLUSION

A taphonomic history results in a fossil assemblage which may poorly reflect the quantitative properties of the biotic community from which the fossils derived. Taphonomic processes sometimes mimic and other times obfuscate their respective effects, thereby rendering the writing of taphonomic histories difficult. While a terminology has been developed to characterize the complexities of taphonomy (Appendix), that terminology has become unwieldy and as a result is not regularly utilized in its entirety. I have attempted here to reduce the complexities of taphonomic processes to a small set of general processes and to align these general processes with their respective general effects. This simple framework guides us towards recognition of data requisite to taphonomic analyses. Perhaps the esoteric results of control studies can eventually be integrated into this framework and ultimately lead to a more holistic theory of taphonomy.

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APPENDIX

Taphonomy Terminology

actualism: "the methodology of inferring the nature of past events by analogy with processes observable and in action at the present" (Rudwick 1976:110)

actuopaleontology: (1) the study of modern organisms and environments for application to paleontological problems (Kranz 1974b) (see also Schafer 1962; Richter 1928); (2) emphasizes the idea that understanding the post-mortem history of one fossil group often requires knowledge of the life histories of associated and interacting organisms (Lawrence 1968); (3) "the application of the uniformitarian principle to

paleontological problems" (Warme and Hantzschel 1979); (4) see "neotaphonomy" and "neontology"

allochthonous: a fossil assemblage which has been transported from the area where the represented animals died and presumably lived

anastrophe: a catastrophe of limited scope and area, generally producing mass mortality in the affected area (Kranz 1974b)

anataxic: factors operating on buried bones, including: re-exposure or disinterment, weathering, mineralization, transport after re-exposure

archaeofauna: faunas recovered from archaeological sites (Grayson 1979); see also "paleontological fauna" and "local fauna"

attrition: loss of fossil information by non-preservation (after Lawrence 1968)

attritional mortality: diachronic death assemblage; the deaths of different aged animals over a prolonged period, that indirectly reflects the age-specific survivorship of a population (Voorhies 1969; Gifford 1981)

autochthonous: (1) a fossil assemblage which is found where the represented animals died and presumably lived; (2) fossils which experienced life, death and burial within the same place or locale

biocoenose: (1) the life assemblage of organisms; (2) consists of a living population; (3) "a biocoenose encompasses a biotape and a community of all organisms living in it" (Schafer 1972); (4) an ecological unit (or living community) consisting of an integrated living congregation of diverse organisms with both biotic and abiotic characteristics

biostratinomy: (originally biostratonomy) (1) the study of preburial taphonomic factors, e.g., those processes affecting an organism between death and final burial (Lawrence 1979a); (2) the study of pre- and syn-burial interrelations between dead organisms and their external environment (Lawrence 1968); (3) see "perthotaxic"

catastropic mortality: (1) synchronic death assemblage; (2) a representative sample of all living age classes killed more or less instantaneously, forming a "snapshot" of a living population structure (Voorhies 1969; Gifford 1981)

chrisocoenosis: post-mortem use of bones by humans (A. S. Gilbert 1979); i.e., the fossil assemblage created by post-mortem use of bones by humans

coprocoenosis: a fossil assemblage derived from scats and owl pellets (Mellet 1974)

diagenesis: (1) the study of post-burial taphonomic factors, i.e., between burial and recovery (Lawrence 1979b); (2) study of the post-entombment histories of organic remains (Lawrence 1968); (3) "fossildiagenese" (Muller 1953); (4) see also "anataxic" and "taphic"

disarticulation: the generic process and result of loss of anatomical integrity (Hill 1979b); see also "dispersal" and "scattering"

dispersal: the generic process and result of spatial movement of individual skeletal elements from a single organism (note that two elements may become more, or less, spatially contiguous) (Hill 1979b); see also "disarticulation" and "scattering"

equifinality: the property of allowing or having the same effect or result from different events (Webster's Third International Unabridged Dictionary)

ethnoarchaeology: study of living peoples with the aim of elucidating archaeological problems; i.e., a discipline with the goal of establishing and clarifying the relationships between material vestiges of human behavior and the living systems which generate them (Gifford 1977)

fauna: some specified set of animal taxa in close spatial and temporal association; usually qualified by some geographic, temporal and/or taxonomic criterion (after Odum 1971:366-367) (see also Tedford 1970)

fossil: (1) any specimen demonstrating physical evidence of the occurrence of ancient life; generally distinguished from Recent or non-/sub-fossil remains on the basis of its (the fossil's) geologic mode of occurrence (Schopf 1975); (2) the identifiable remains of (once) living organisms or of their activities preserved in the sediments by natural processes (Finks 1979); (3) any contemporary trace or remain of an organism that died at some time in the past (Matthews 1962)

fossil assemblage: an aggregate of individual elements (that interact with various modification agents in statistical fashion, with considerable potential for variation in traces they ultimately may bear) (Gifford 1981)

fossilization: the maintenance or alteration of chemical properties of organic materials by natural processes (Finks 1979)

fossil record: that contemporary set of fossils in some specified geographic space and geologic context (Lyman 1982); see also "local fauna"

fragmentation: mechanical disassociation of skeletal parts

liptocenosis: remnant (fossil) assemblage (Rolfe and Brett 1969)

local fauna: the fauna represented by one or several geographically, geologically and taxonomically similar fossil samples; i.e., may be represented by fossil samples from a single site or several sites in close geographic and stratigraphic association (not necessarily representative of a biocoenose, and not necessarily implying any paleoecological reality) (Tedford 1970); see also "fossil record"

mineralization: see "fossilization"

neontology: the "paleontology" of living animals including the "paleoecology" of modern environments (Warme and Häntzschel 1979); see also "neotaphonomy" and "actuopaleontology"

neotaphonomy: involves relevant experimentation or observations of the condition of modern vertebrate remains in closely defined environments, designed to test taphonomic conjectures and to suggest consequences for paleoecological interpre-

tation not visible in the fossil record such as the absence of a taxon or the structure and composition of a paleocommunity from certain kinds of fossil remains (Hill 1978)

oryctocoenose: remains that were found together in an outcrop (Lawrence 1979c)

paleoecology: (1) the study of environmental relations of fossil organisms between their birth and death (Lawrence 1968); (2) a discipline focusing on interrelationships which occurred in the geologic past between living organisms and their surroundings (Lawrence 1971)

paleontological fauna: (1) "the maximum geographic and temporal limits of a group of organisms sharing a suite of common species" (as evidenced by the fossil record) (Tedford 1970); (2) faunas recovered from paleontological sites; (3) see also "archaeofauna" and "local fauna"

paleotaphonomy: observations on fossil assemblages (Hill 1978); see also "neotaphonomy" and "taphonomy"

pedoturbation: various processes of homogenization (or haploidization), which impede soil horizon formation; soil mixing (Wood and Johnson 1978); may be mechanical or chemical (faunal-turbation; floral-turbation; cryoturbation; graviturbation; argilliturbation; aeroturbation; aquaturbation; crhstalturbation; seismiturbation)

perthotaxic: taphonomic factors which operate between the time of an organism's death and the time of its burial, including but not limited to scavenging and weathering; see also "biostratinomy"

perthotaxis: a death assemblage with corpses in various stages of decomposition (Clark and Guensburg 1970)

petrification: (1) "cellular permineralization": permeation of cells and interstices (not replacement) by mineral matrix at or very soon after deposition (Schopf 1975)

preservation: (1) "duripatric (hard part) preservation"; original hard parts are preserved due to resistance to oxidation and physical damage (Schopf 1975); (2) "authigenic preservation"; fossil is encased by cementing minerals which preserve surface configuration of organic parts while internal organization is lost or degraded (Schopf 1975)

proximal community: the species of a community which lived in close spatial proximity to the site of the deposition of their remains (Shotwell 1955); see also "autochthonous"

quarry site: localized concentrations of fossil bones; vary greatly in density of materials and total volume; vary in degree of representation of biocoenose (Shotwell 1955)

scattering: the increase in dispersion of skeletal parts (Hill 1979b); see also "dispersal" and "disarticulation"

signature criterion/pattern: "a criterion that is constant and unique and that discriminates one modifying agent or set of agents from another" (Binford 1981; Gould 1980)

sullegic: factors influencing collection; i.e., whether or not a particular fossil is collected; includes: area of site or site chosen, sampling design (where you collect), collection procedures (e.g., hand-pick versus screening (mesh size) versus flotation) (Meadow 1981)

taphic: factors determining whether or not a bone will be buried; the when, where, how and why of burial

taphocoenosis: (taphocoenose) assemblage of organic materials which are buried together (Lawrence 1979c)

taphonomy: (1) the science of the laws of embedding or burial; the study of the transition, in all details, of organics from the biosphere into the lithosphere (Efremov 1940); (2) the study of differences between a fossil assemblage and the community(ies) from which it derived; the nebulous region of conjecture constituting hypothetical assertions about the causes of the observed bias in fossil assemblages (Hill 1978)

thanatic: (1) factors or variables pertaining to the death of an organism; (2) causes of death; (3) circumstances inducing death among individuals of a biocoenose (Clark and Kietzke 1967)

thanatocoenose: (1) the death assemblage derived from a biocoenose (biocoenose = thanatocoenose = fossil assemblage); (2) may not be from one but several communities (Shotwell 1955); (3) organisms that died together (Lawrence 1979c)

transport: loss of fossil information by physical movement of fossils away from the site of the original biocoenose (adapted from Lawrence 1968)

trephic: (1) factors incident to curating and identifying a specimen which determine whether a particular fossil becomes available for analytic use (i.e., becomes or provides data) (Clark and Guensburg 1970); (2) includes determining which bones that were recovered are identified/recorded (skill of analyst), analytic procedures (sampling), to publication format (NISP or MNI or both) (Meadow 1981)

weathering: chemical and mechanical alteration of bone microstructure prior to burial (Behrensmeyer 1978)

NEWS AND COMMENTS

SOCIETY OF ETHNOBIOLOGY ELEVENTH ANNUAL CONFERENCE MEXICO CITY, MEXICO

Bob Bye, Chairman of the local committee for the 11th Annual Ethnobiology Conference has released a preliminary announcement of dates and arrangements for our next meeting to be held in Mexico City in March, 1988. This announcement of tentative plans and events (with a questionnaire so the committee can better know of your interests) was mailed in late May, 1987, to all members of the Society of Ethnobiology and to those on our supplemental mailing list. If you did not receive a copy, you can obtain one by writing to either Bob Bye or Jan Timbrook at the addresses given below. We give here an abbreviated version, without the questionnaire, of that announcement.

11th Annual Ethnobiology Conference

LOCATION: Mexico City, MEXICO (Centro Interamericano de Estudios de Seguridad Social = CIESS)

DATES: Meetings: 17 - 19 March 1988
Field Trips: 20 - 23 March 1988

PRINCIPAL HOST: Jardin Botanico, Instituto de Biologia, Universidad Nacional Autonoma de Mexico

REGISTRATION (non-Latin Americans): Regular - $30 US dollars
Students - $20 US dollars

BANQUET: $7 US dollars. Many regional dishes and various ethnobiological edibles and beverages, depending upon availability.

ORAL PRESENTATIONS: 2 full days / POSTER SESSIONS: 1 full day

ETHNOBIOLOGY BOOK FAIR (featuring Mexican publications)

FIELD TRIPS:

1—one day (20 March): $10 US dollars
(a—Teotihuacan (archaeological visit); b—Malinalco (archaeological visit); c—Milpa Alta (*Opuntia* cultivation)

2—three to four days (20 - 23 March): $100-150 US dollars
(a—Veracruz (contemporary ethnobotanical studies in humid tropical zone); b—Michoacan (contemporary ethnobiological studies in temperate zone; Purepecha of Patzcuaro); c—Tehuacan, Puebla (archaeological area of dry highlands); d—Sierra del Norte, Puebla (contemporary ethnobiological studies in temperate-subtropical zone).)

OTHER POSSIBLE ACTIVITIES: 1—Mercado Sonora (Mexico City) visit (medicinal plants); 2—Museum of Anthropology; 3—Templo Major and National Palace (murals of Diego Rivera)

HOUSING AND FOOD: During the three days of activities in Mexico City on site housing and food service is available at CIESS:

Room: $7.50 US dollars per person per day
Food: $6 US dollars per person per day.

TRANSPORTATION: Taxi, collective and metro services are available at the airport to the University area. CIESS is located a few kilometers to the west of the University and scheduled transportation will be arranged for each day.

LANGUAGE: Papers, posters and "platicas" will be in Spanish and English. Simultaneous translation will be available in the main auditorium. Bilingual hosts will be available to groups of non-Spanish speaking ethnobiologists.

TRAVEL: All roads, railroads and air routes lead to, start from and end in Mexico City. Many North American and Mexican airlines have many flights arriving and leaving Mexico City throughout the day and night. There are some good bargains for people departing from certain major US cities and making reservations ahead of time.

ENTRANCE INTO MEXICO: Most US and Canadian citizens need only a proof of citizenship and a tourist card (available from tourist agencies) to enter Mexico. Naturalized citizens or citizens of other countries need to check specific requirements in advance.

Additional information can be obtained from: Robert Bye, Jardin Botanico, U.N.A.M., Apdo. Post. 70-614, 04510 Mexico, DF, MEXICO or Jan Timbrook, Santa Barbara Museum of Natural History, 2559 Puesta del Sol Road, Santa Barbara, CA 93105 USA.

BARBARA LAWRENCE PRIZE ANNOUNCED

The Society will award a prize in honor of Barbara Lawrence for the best paper submitted by a student for presentation at the 11th Annual Meeting. The competition is open to any member who considers themself a student and has not held the PhD degree at the end of the preceding summer session. The paper can be presented in an oral or a poster session and will be considered for publication in the *Journal of Ethnobiology*.

Manuscripts submitted for this competition should be single authored only; joint efforts will not be considered. Manuscripts are judged solely on quality, originality, and presentation of research. They should follow the *Journal of Ethnobiology* format and should be sufficiently precise and documented to enable the reviewing committee to judge their merits. Manuscripts are limited to eight double-spaced, typed pages, including a required abstract but excluding copies of figures, tables, and references.

Please include a cover letter indicating that you are a Society member and meet the criteria listed above and send it and your paper early enough that they will arrive at their destination on or before December 1, 1987. Applicants from the USA and Canada may send material to the 11th Annual Conference Committee, c/o Jan Timbrook, Department of Anthropology, Santa Barbara Natural History Museum, 2559 Puesta del Sol Road, Santa Barbara, CA 93105, USA. Those from Mexico, other Latin American countries or elsewhere may also use this address or, if they prefer, may mail their material to the 11th Annual Conference Committee, c/o Dr. Robert A. Bye, Jr., Jardin Botanico, U.N.A.M., Apdo. Post. 70-614, 04510 Mexico, DF, MEXICO.

SOCIETY NEWS

Steven D. Emslie, co-founder and past Secretary/Treasurer of the Society and *Journal of Ethnobiology*, was elected an honorary member of the Board of Trustees at the 10th Annual Conference, Gainesville, FL.

SHORT COMMUNICATIONS

With this issue the editorial staff of the *Journal* is introducing a new feature—**SHORT COMMUNICATIONS.** Robert Bye, a member of the Board, kindly agreed to write the initial article as an example of the type of material we are seeking for this column. For the time being, kindly send submissions for **SHORT COMMUNICATIONS** to the Editor of the *Journal of Ethnobiology*.

Datura and Castaneda.—As I reevaluate *Datura* in ethnobotanical sources, I am taken back to the period of my initial interests in ethnobotany during which Carlos Castaneda's "classic" book, *The Teachings of Don Juan: A Yaqui Way of Knowledge* (1969, Ballantine, New York), appeared. According to the text, Carlos experimented with "jimson weed" four times. The species, *Datura inoxia*, was not documented by a voucher specimen, hardly surprising if one accepts the hypothesis of Richard de Mille (1978, *Castaneda's Journey: The Power and the Allegory*, Capra Press, Santa Barbara, CA) that Carlos-Ethnography is a myth and that no legitimate field work was done. Where did Castaneda obtain the clue to inquire about *Datura*?

According to de Mille (1978:114), Castaneda was asked by Michael Harner who came across a reference to the Yaqui use of a *Datura* ointment rubbed on the stomach to see visions. Unfortunately, Harner lost the reference. Not only does de Mille doubt Harner's claim that such a reference exists, but the investigative writer also quotes Dr. Edward Spicer who states that he knows " . . . of no information or reference concerning Yaqui using *datura* (sic!)." In his revised edition, de Mille (1978:115) cites one possible reference (José Peréz de Barradas, 1957, *Plantas Mágicas Americanas*, Instituto Sahagún (Madrid), pp. 239, 310-312) which is based upon Victor Aloisius Reko, brother of the respected Mexican botanist, Blas Pablo Reko.

I suggest that two more readily available and standard Mexican publications were probably the sources, these being the books of Maximino Martínez: 1) *Las Plantas Útiles de la Flora Mexicana* (first edition 1928; third edition 1959, Ediciones Botas, México). These books, which are widely distributed, would surely be consulted by anyone working on useful and medicinal Mexican plants.

Martínez (1959:572) reports *Datura stramonium* L. "and other species" (including *D. inoxia* from Sonora) as the ***toloache.*** The Yaquis "and other tribes of the north" employ the plant for various purposes. An ointment prepared from the ground seeds and leaves of the plant is smeared onto the stomach after which the subjects become intoxicated and see visions. ("Ademas hacen una especie de unguento con semillas y hojas molidas, para untarselo en el estomago, con lo cual tambien se intoxican y ven visiones.")

The source of Martínez' report is unknown. It may be related to V. Reko's writings on ***toloachi.*** Three of Reko's articles (1928, 1932 and 1936) cited by I.K. Langman (1964, *A Selected Guide to the Literature on the Flowering Plants of Mexico*, University of Pennsylvania Press, Philadelphia, p. 621) discuss ***toloachi.*** Reko (1936, *Magische Gifte*, Ferdinand Enke Verlag, Stuttgart, pp. 80-83) discusses the uses of ***toloachi*** (unidentified) in Veracruz and Guanajuato as well as among the Mayo and Yaqui Indians of Sonora.

Some people have questioned the reliability of V. Reko's work and writings. Most botanists of Mexico were active in their formation of the Mexican Society of Botany and deposited their books and specimens at the Institute of Biology of the National University of Mexico. A search at this institute failed to reveal any original notes, herbarium specimens or researchers familiar with his botanical contributions.

Five species of *Datura* occur in southern Sonora and could be candidates for ***toloache.*** Two widely distributed species with documented uses in northern Mexico and adjacent United States are *D. stramonium* L. and *D. wrightii* Regel. Another common species in the area is *D. discolor* Bernh. Two species restricted to the northwestern coastal plain

and barrancas that cut into the Sierra Madre Occidental to the east are *D. lanosa* Barclay ex Bye and *D. reburra* Barclay.

Consequently, it is difficult to verify the use of *Datura* by the Yaqui Indians and to determine the species reputedly employed. The reference that stimulated Harner to have Castaneda query Don Juan about *Datura* was probably one of the classic economic botany books by Martínez. Also, the report of *Datura* use among the Yaquis may have originated from Victor Reko. Because of the lack of voucher specimens and corroborative studies, the use and identification of ***toloache*** in Sonora need to be investigated.

The thesis of de Mille's is that Castaneda's Don Juan is fiction rather than ethnographic fact. Regardless of the final verdict, Donald Lee Williams (1981, *Border Crossings: A Psychological Perspective on Carlos Cataneda's Path of Knowledge*, Inner City Books, Toronto) argues that the process of the path of knowledge is quite real. His comments on *Datura* from a Jungian viewpoint add another dimension to ***toloache*** in the human experience.

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NOTICE TO AUTHORS

The *Journal of Ethnobiology* accepts papers on original research in ethnotaxonomy and folk classification, ethnobotany, ethnozoology, cultural ecology, plant domestication, zooarchaeology, archaeobotany, palynology, dendrochronology and ethnomedicine. Authors should follow the format for article organization and bibliographies from articles in this issue. All papers should be typed double-spaced with pica or elite type on 8½ x 11 inch paper with at least one inch margins on all sides. The ratio of tables and figures to text pages should not exceed 1:2-3. Tables should not duplicate material in either the text or graphs. All illustrations are considered figures and should be submitted reduced to a size which can be published within a journal page without further reduction. Photographs should be glossy prints of good contrast and sharpness with metric scales included when appropriate. All illustrations should have the author(s) name(s) written on the back with the figure number and a designation for the top of the figure. Legends for figures should be typed on a separate page at the end of the manuscript. Do not place footnotes at the bottom of the text pages; list these in order on a separate sheet at the end of the manuscript. Metric units should be used in all measurements. Type author(s) name(s) at the top left corner of each manuscript page; designate by handwritten notes in the left margin of manuscript pages where tables and graphs should appear.

If native language terminology is used as data, a consistent phonemic orthography should be employed, unless a practical alphabet or a more narrow phonetic transcription is justified. A brief characterization of this orthography and of the phonemix inventory of the language(s) described should be given in an initial note. To increase readability native terms should be indicated as ***bold-face italics*** to contrast with the normal use of *italic* type for foreign terms, such as latin binomials. If necessary, the distinction between lexical *glosses*, i.e., English language approximations of a term's referential meaning, and precise English equivalents or definitions should be indicated by enclosing the gloss in **single quotation marks**.

Authors must submit two copies of their manuscript plus the original copy and original figures. Papers not submitted in the correct format will be returned to the author. Submit your manuscripts to:

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Journal of Ethnobiology
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NEWS AND COMMENTS

Individuals with information for the "News and Comments" section of the *Journal* should submit all appropriate material to Eugene Hunn, Department of Anthropology, DH-05, University of Washington, Seattle, Washington 98195.

BOOK REVIEWS

We welcome suggestions on books to review or actual reviews from the readership of the *Journal*. Please send suggestions, comments, or reviews to Terence E. Hays, Department of Anthropology and Geography, Rhode Island College, Providence, Rhode Island 02908.

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VOLUME 7, NUMBER 2 WINTER 1987

The beaches encircling the Mediterranean island of Cyprus provide a perfect setting for a sketch in the sand. But any marks I etch with a stick are resolutely obliterated by the sea, which seems to favor only rippled designs. It is easy to become mesmerized as the rhythmic movements and sounds of the lapping water lull me into a trance-like state. Instead of empty waves I envision waves bearing ancient wooden ships full of people on their way to settle this small island some eight or so millennia ago. My shoreline design takes on the appearance of changing patterns of plants that have cloaked the landscape through time. As the vulnerable sketch is slowly erased by the waves, one ponders if there might be certain parallels between the relentless wave action and any role humans played in the vegetation history of the island.

While sorting charred plant parts recovered from the A.D. 100-365 Roman Age site of Kourion (Curium), I began to ponder the patterns of flora on Cyprus. On excursions to collect modern plants in the lowland island setting where this ancient city is perched overlooking the sea, always in the back of my mind was the question, "Is the current vegetation similar to that which was available to the Romans?" Knowing that Kourion and other prehistoric ruins on Cyprus have preserved only limited clues to ancient floral choices, it seemed important to explore the history of the island to shed light on the matter.

There may be few of the present-day vegetation patterns that an ancient settler would recognize. Although natural forces such as changing moisture and temperature regimes have had a lot to do with this, the forces applied, consciously and unconsciously, by humans have been of no less impact. For millennia people have cut down the trees of Cyprus to fuel the smelters and build the ships that transported copper and other important trade goods to large and small Mediterranean centers of commerce. Today only 17% of the island is classified as woodland, yet in the 3rd century B.C. a writer described the island as completely forested. People have also cleared extensive sections of land for grapes; today a trip into the Troodos Mountains reveals completely terraced slopes bearing vineyards for as far as the eye can see. Domestic sheep, goats, cows and pigs have grazed the land for much of the period that people themselves have been on the island. And all the while new plants from foreign lands were brought to Cyprus, starting very early with crops such as barley *(Hordeum)* and wheat *(Triticum)*, and continuing through the ages, until today many villagers seem to have no clear sense of the island's native floral identity.

Although in many parts of the world vegetation history is inferred from pollen that is recovered from continuously wet places such as bogs or lakes, Cyprus offers few such likely situations where one might look for a long-term pollen record. In lieu of this we can offer informed speculation on what plants were probably available to the ancient Kourion folk. R. D. Meikle, the scholar who wrote the two volume *Flora of Cyprus*, has given much thought to the island's floral history and to the nature of a lowland climax

vegetation. His ideas derive from visits to relict areas that provide hints of natural lowland vegetation patterns. Where today the only trees that one sees when standing on the Kourion ruins are domestic olive *(Olea)* and carob *(Ceratonia)*, it seems likely that once a thin forest of pine trees *(Pinus brutia)* and smaller trees or shrubs of juniper *(Juniperus phoenicea)* and oak *(Quercus coccifera)* would have been within view. Where now a limited number of widely-spaced small shrubs armed with spines and stickers predominate, a diversity of tall and short shrubs including various species of showy-flowered *Cistus* and fragrant members of the mint family (Labiatae) probably prevailed. What perennial and annual taxa were common is anybody's guess; currently one finds an abundance of thistles *(Echinops, Centaurea)* and a species of asphodel *(Asphodelus)* scrupulously avoided by grazing sheep and goats. The list of trees and shrubs that could be expected in mesic habitats is long; the edges of the nearby dry Kouris riverbed today harbors only low shrubs and barren ground. The likely conclusion one comes to is that humans and nature together have fostered vegetation changes on Cyprus that will be difficult to reconstruct over the long run. The local vegetation viewed 15-18 centuries ago by the Romans at Kourion could well have been different indeed.

Even as we formulate hypotheses on the nature of past vegetation the business of carefully confirming the identity of the modern floral collections must go on. So, eventually my large box of modern Kourion area plants had to be maneuvered among the crowds on the London subway, and then lugged some distance to Kew Herbarium so that species identifications could be verified in a repository well represented by Mediterranean specimens. On this journey it occurred to me that most people would take what I was carrying at face value—a box of dried and flattened plants. But I considered this bulky box to be a historical document that in part chronicled human activities. I carried domestic olive and carob sandwiched among a host of thorny and prickly resistors to grazing. Packed inside was a piece of joint-pine *(Ephedra)* that a local goat had hungrily sought from me after I extricated the specimen from deep within a tangle of barbed wire. What was missing from my box were the pines, junipers and oaks that once might have provided a tall upper story appearance to the landscape, and the diversity of mesophytic species that may no longer be in the region. Who knows what other taxa were missing. We are hopeful that with time the site of Kourion will relinquish a number of secrets regarding those plants present in the area during Roman times.

For one whose primary professional activities have been centered in the American Southwest, where the nature of human impact on vegetation seems mild by comparison, working with an ancient plant record in a long-occupied area of the Old World was an eye-opening experience. But while I have emphasized contrasts through time, I was reminded by excavation director David Soren that as things change, so do they remain the same. One perceives continuity between the ancient veneration of trees planted and tended in a sacred grove at the Iron Age Sanctuary of Apollo near Kourion, and a huge living tree in the nearby town of Paphos that today has hundreds of small rags tied to its branches by people with special requests. These modern shreds of cloth subtly reveal how certain cultural points of view may be retained and rewoven to fit a (partly self-induced) changing natural resource base.

The hints offered from archaeological sites about ancient floral choices on Cyprus offer a foundation for understanding the island's vegetation history. Observation of modern relict areas of plants provides insight helpful in speculating on the effects of thousands of years of domestic grazing, cultivation, and tree-cutting. Together they suggest that the vegetation available to the Romans at Kourion represents a state somewhere along a long continuum of change, perhaps not much more stable than my modern designed sketched into the shortline sand, persisting only briefly before the sea reclaims it.

Karen R. Adams

J. Ethnobiol. 7(2):123-135 Winter 1987

A PALYNOLOGICAL APPROACH TO A CHRONOMETRY PROBLEM ON THE COLORADO PLATEAU

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ABSTRACT.—Application of the Colorado Plateau Pollen Chronology (CPPC) addresses the issue of contradictory chronological reconstructions provided for the site of Nuvakwewteqa by Upham (1980, 1982), Brown (1982) and Coinman (1984). Coinman's concerns regarding assumptions basic to Upham's chronometry are supported by palynological information. Palynological support for an alternative chronometry, however, is frustrated by the distribution of samples with insufficient pollen for analysis.

INTRODUCTION

From early 1977 to late 1982 execution of the Chavez Pass Archaeological Project was administered through the Department of Anthropology at Arizona State University. As a result, the sediment samples collected during those years for pollen study as aspects of the surveys, tests, salvage and room excavation operations performed at the site of Nuvakwewtaqa and in its environs are curated at the Palynology Laboratory at ASU. Among other products, the Chavez Pass Archaeological Project has generated a distinctive processual model to account for the flourescence and abandonment of 14th century Pueblo communities on the Colorado Plateau (Upham 1980, 1982). This model has had significant impact upon modern assessments of Puebloan archaeology and culture (e.g. Cordell and Plog 1979; Upham 1983, 1984; Upham and Plog 1986). In some ways, however, specifics of the model are critically dependent upon interpretations Upham has made of the chronometry of cultural events at the site of Nuvakwewtaqa.

Normally, application of methods that allow development of "pollen dates" for archaeological contexts would be considered superfluous for a Colorado Plateau site as large and as complex as Nuvakwewtaqa. Generally speaking, the large, multi-storied peublos of the region contain an abundance of the Anasazi decorated ceramic styles that are well cross-dated to tree-ring patterns. Nuvakwewtaqa, however, is classified as a Sinagua culture pueblo (Wilson 1969; Batcho 1977; Upham 1980). Well cross-dated decorated ceramic materials are rarer in its artifact inventory and chronometric precision is far more difficult to achieve than is often the case in the American Southwest.

Grappling with this problem in the course of attempts to reconstruct the process of the establishment, development and abandonment of Nuvakwewtaqa, Upham (1980, 1982) employed a chronometric method (Mean Ceramic Dating, or MCD) that had been first documented as applicable to Colonial Period sites in the Eastern United States (South 1972). The processual reconstruction Upham achieved, which is in some ways critically dependent upon the MCD antiquity estimates for sites in the Chavez Pass district and for various portions of Nuvakwewtaqa, challenges conventional archaeological wisdom in a number of respects (McGuire 1983). Not surprisingly, perhaps, work performed at Nuvakwewtaqa subsequent to Upham's analysis has explored the possibility of error in Upham's chronometry. Brown (1982) suggested an alternative chronometry for events at the site based on chronometric dates and Coinman (1984) tested the applicability of

an alternative numerical method for estimating the antiquity of ceramic assembledges at this site.

Both alternative chronometries for the events of the process suggested by Upham are argued to be more consistent with radiocarbon and tree-ring information that was not available when Upham developed his processual model. Also, some of the assumptions that Upham employed in his application of the MCD method have been challenged. Thus Brown's and Coinman's work suggests that Upham's processual explanation of the development of the Western Pueblo from the 14th century to the ethnographic present may be flawed by chronological errors.

The CPPC, or Colorado Plateau Pollen Chronology (Schoenwetter 1970) was created to provide both a means of estimating the antiquity of archaeological materials directly associated with pollen records of the last 2000 years in this region, and to provide a reconstruction of paleovegetation responses to effective moisture variations over this period of time. It is not, and was never designed to be, a regional pollen chronology in the traditional sense of the Northwest European pollen sequence or Hall's (1985) synthesis of the Southwestern Quaternary palynological record. For one thing, the CPPC can *only* be applied to pollen samples collected from archaeological site deposits (see below). For another, the CPPC does not identify episodes of vegetational change; it identifies the temporal parameters of eposides in a sequence of changes in a single ecosystem variable: effective moisture. Critics of the CPPC assert that it must be biased by the realities of episodic sedimentation, poor preservation and questionable origin of the pollen of archaeological site deposits. The point is well taken. However, a number of independent tests of the chronology have been performed since it's publication, (e.g., Schoenwetter 1976; Rosenberg 1976; Buge' and Schoenwetter 1977; Scott 1977, 1978; Peterson 1983), which it has withstood, and a remarkable correspondence has been recognized between the antiquity of episodes of relatively higher and lower effective moisture conditions in the CPPC and those recognized on the basis of decadal averages in tree-ring growth records from the Colorado Plateau (Schoenwetter 1971; Dean in press). It may well be true that, in theory, the CPPC cannot be useful for the purposes for which it was designed. But the applicability of the CPPC is supported by evidence, and the assumption that traditional palynological theory in fact applies to the pollen of archaeological site deposits is not. Other palynological approaches to the problems of paleovegetation and paleoecosystem conditions reconstruction have been presented since publication of the CPPC (e.g., Euler *et al.* 1979; Hevly 1981), but the CPPC is the only palynological method advanced for the region that has the purpose of dealing with specifically chronometric problems. Application of the CPPC to assist resolution of the chronometric debate that has arisen at Nuvakwewtaqa, then, seems appropriate.

IMPORTANT CONSIDERATIONS

Character of the CPPC.—The Colorado Plateau Pollen Chronology is structurally distinct from most pollen chronologies in two significant ways. First, most pollen chronologies identify a sequence of pollen zones, each of which is characterized by a particular and unique set of palynological features. The CPPC identifies a sequence of temporal episodes during which effective moisture levels were equivalent to, or were lower, or were higher than a specified standard. The palynological character of an episode of lower effective moisture level which occurs early in the sequence is not different from that of an episode of lower effective moisture level which occurs late in the sequence. Assigning the pollen record of a particular sample to a particular temporal episode, then, requires consideration of it's contextual and stratigraphic relationship to other pollen records from the site and the temporal positions of associated artifacts.

Second, it is unusual because the chronological significance of a pollen statistic or relationship is normally a function of the character of that statistic or relationship, but this is not true of the CPPC. The pollen statistic having chronological value in the CPPC is the Adjusted AP frequency (Adj. AP%) value. However, in any given pollen spectrum this pollen statistic only has chronological value if the sample's stratigraphic and contextual relationships to other pollen records from that site can be evaluated, and if the associations it has to the archaeological records of the site are known. This body of information is required for assessment of the probability that the pollen record is a function of human behavior rather than the effective moisture level of the regional ecosystem.

In fact, the principal difficulty that arises when application of the CPPC is considered is not the technical one of determining a sample's Adj. AP% value, but the requirement to assess the likelihood that the pollen record one observes is an artifact of human behavior rather than an expression of the natural pollen rain. Aspects of archaeological and of palynological theory that are both long-standing and securely evidenced compel recognition that the sorts of pollen records to which the CPPC may be applied have a high prior probability of being behaviorally induced (Hevly 1981; Schoenwetter 1986). Yet the CPPC has been tested many times and it's success rate is no lower than is true of pollen chronologies based upon non site-context data. Apparantly, the CPPC is a chronometric method that works for the purposes for which it was designed, but which works far more effectively than existing palynological theory can account for. This has, apparently, created suspicion that application of the CPPC is either an unscientific excercise or an illusory one, for it has not been widely used.

As author of the CPPC (and as one who has received no more theoretical enlightenment from the progress of these tests than seems the case for others), I well appreciate and understand that suspicion. I also appreciate the value of caution in application of methodologies that have no clear theoretical rationale. For better or worse, however, archaeological research traditionally adopts the pragmatic view that the appropriate test of a method is not whether we understand it but whether or not it works to achieve what it is designed to achieve. Within its expressed limits (the probability of an innacurate chronometric estimate for any given sample = 12.5%) the evidence at hand is that the CPPC works well.

Site Structure and Chronometries.—The most recent succinct description of the site (Upham and Plog 1986) states:

> "Nuvakwewtaqa, a large site (more than 1,000 rooms) comprises three primary roomblocks, numerous smaller roomblocks and extramural features, as well as a vast system of agricultural terraces, linear grid systems, check dams, and isolated fieldhouses . . . Kivas are associated with each of the roomblocks, as are protected plaza areas. A Great Kiva is located in the largest of the three contemporaneous roomblocks and a possible ballcourt is nearby."

There are two principal habitation areas at the site, referred to as the North and the South Pueblos, with one roomblock at the former and two at the latter. Upham's information on the North Pueblo was derived from analyses of the surface collections of artifacts, salvage information on the highly disturbed burials, and some test pit and soil auger core data. He has suggested (Upham 1982:169, 190; Upham and Plog 1986:226) that all the roomblocks were occupied by A.D. 1100 but the principle occupation of the South Pueblo did not begin until a period of expansive construction of the South Pueblo was initiated about A.D. 1270. Upham concluded that the North Pueblo was abandoned about A.D. 1315, though occupation continued at the South Pueblo at least until 1425.

Coinman (1984) considered the results provided by additional excavation of rooms and other features at the North Pueblo, the body of new radiocarbon and tree-ring information provided by Brown (1982), and the vertical and horizontal distribution patterns of architectural features and associated decorated ceramic type clusters. She concluded that the North Pueblo territory was occupied some centuries prior to construction of the roomblock, but the roomblock itself was established no earlier than A.D. 1150. She argues that both the North and the South Pueblos are likely to have been constructed, occupied and abandoned about the same time, and she suggests that Upham's chronological reconstruction may be inaccurate because it is based upon weak assumptions and, possibly, the use of an inappropriate chronometric method (MCD) (Coinman 1984:121-123).

The two assumptions involved relate to the stratigraphic situation Upham considered adequately evidenced for the site. One is that the sequential deposition of differentiable middens occurred uniformly and coincidentally at both the North and South Pueblos (Upham 1982:169). The other is that the stratigraphic relationships of pottery assemblages recovered from a single room at the South Pueblo can be generalized to reflect the temporal relationship of ceramic assemblages recovered from other locations at the site and other sites in the district (Upham 1982:193).

The units of Upham's stratigraphic analysis are those I recognized for the site on the basis of inspection of exposures revealed by looters searching for collectible artifacts (Schoenwetter 1978). Five of the eight depositional strata I currently recognize (Units C-G of Table I), are middens that are differentiated by context, superposition, color and artifact density. Coinman properly recognizes that the criteria for definition of the units of Upham's stratigraphic sequence (superposition, color and artifact density) do not assure the comparability and contemporaneity of similar deposits across the site or even within the same part of the site. She therefore suggests that Upham's analysis lacks *a priori* credibility.

Upham faced the problem of establishing a relative ceramic chrononology for the Chavez Pass District at a time when the bulk of available information about the ceramic types associations was derived from surface collections. However, opportunity arose in the 1978 season to excavate a 2x2 meter test at the South Pueblo (east roomblock) in a room that had mostly been in-filled through the intentional disposal of trash. This excavation, at locus 33.8S, yielded a relatively large collection of decorated pottery assemblages in stratigraphic superposition that could be related to the mixed middens, red-brown midden and younger brown midden units of the site's stratigraphy. Upham documented the credibility of application of the MCD method with this case. He then confirmed that none of the information available from test pit excavations conflicted with the chronometry so devised. He assessed that result as justification for generalization of the chronometry from the 33.8S locus to the site as a whole and the Chavez Pass District, as well. Coinman's analysis of the vertical and horizontal distributions of the ceramic styles subsequently excavated at the North Pueblo provided evidence for a conflicting case. She therefore questioned the stratigraphic assumptions Upham used for determining the chronometry of events at the site or in the Chavez Pass District.

Appropriateness of the CPPC.—The CPPC can be applied to this conflict because field efforts were specifically directed towards the recovery of pollen samples controlled by contextual relationship to the units of the stratigraphic sequence at Nuvakwewtaqa *and* controlled by direct association with ceramic assembladges whose antiquity could be estimated by the MCD method. In 1977 I collected and analyzed pollen samples from exposures of the stratigraphic units and determined that they yielded palynological records that were potentially datable through application of the CPPC (Schoenwetter and Stewart

1978). In 1978 I established test pit excavations in various areas of the site that would allow recovery of both ceramic assemblages and stratigraphically superimposed pollen samples associated with the complete spectrum of stratigraphic units. Upham's excavation at 33.8S constituted another pollen sequence in which the relationship of the pollen samples to both stratigraphic units and ceramic assemblages was controlled. Yet another opportunity was provided when testing and trenching operations were performed at the possible ballcourt feature in 1980. In the latter two cases, however, the pollen samples were collected from profiled exposures after the ceramic assemblages had been collected in arbitrary 10 cm levels that could not individually be precisely correlated to the stratigraphy. The contextual identification of the pollen samples and stratigraphic units of these tests, then, is controlled in the same way as occurred in the other tests identified on Table I. But the association of the pollen records with the ceramic assemblages dated by the MCD method is indirect rather than direct.

A question that some readers may consider pertinent is the potential of the pollen spectra of these samples to be influenced by the occurrence of pollen which was introduced through downwashing subsequent to deposition, or which contaminates the sample for some other reason. The possibility of contamination certainly exists for any sample or set of samples, of course, but there are at least three reasons to accept the operating assumption that the Nuvakwewteqa pollen spectra are not contaminated until evidence is presented to the contrary. First, though downwash is credited as a major mechanism which structures the pollen sequences of archaeological sites (Dimbleby 1985:4-9), it is granted that status only in environments in which soil formation and rainwater leaching processes are quite active. Neither of these conditions occurs at archaeological sites on the Colorado Plateau. Second, one expects 12.5% of archaeological pollen records assigned by the CPPC to particular temporal intervals to be inaccurately dated. Actually, less than 5% can be demonstrated to be inaccurate. The empirically evidenced degree of temporal correspondence of changes in the CPPC with changes in the tree-ring sequence of the Colorado Plateau, and the evidence that the CPPC is regularly expressed under a variety of archaeological context situations, strongly argues that the pollen spectra datable by the CPPC do not display tendencies for contamination that affect chronological application. Third, the prior probability is that the pollen spectra characteristics observed in these samples are behaviorally induced, since the samples are of midden, which is a product of human behavior. If it is demonstrable that this prior probability does not in fact affect the spectra, and they seem datable by the CPPC, it is hardly likely that this is the fortuitous result of contamination processes. To assume so would violate some of the basic principles of biostratigraphic correlation upon which "pollen dating" is based at any location in the world.

Sampling strategy.—The strategy responsible for the collection and analysis of the samples arrayed on Table I focussed on recovery of sample series that offered opportunity to compare the Adj. AP % values of ostensibly contemporary pollen records from different parts of the site with each other, and to compare those of older spectra from the same locus against those of spectra that were necessarily younger. To the degree results were consistent in time and space, they could be evaluated as Adj. AP % values unaffected by human behavior, and thus datable by application of the CPPC. The strategy required study of 74 pollen samples, of which 18 (24%) yielded too few pollen grains for confident evaluation.

RESULTS AND DISCUSSION

Tables II-VII, taken in conjunction with the comparative information provided on Table I, specify the relevant results of this study. (Raw data is on file at the Arizona

State University and also at New Mexico State University.) The consistency of the Adj. AP % pollen statistic values of stratigraphically related samples from the different tests supports rejection of the operating assumption that that statistic is induced by human

Strati-Graphic Unit	Unit Description	416.7N "Ballcourt" Backhoe Trench 1	22S	33.8S	121.3S	150S
A	Culturally sterile superficial layer	27 4104	4105 4104			
B	Artifact-rich slopewash layer		4103	0-10 cm	37	
C	Younger brown midden pitfill			50-100 cm		
D	Younger brown midden	26 25	4102 4101 4100	2 20-90 cm		13 59 58
E	Red-brown midden (sometimes with brown midden lenses)	24 10 23 22 21 20 16	4099 4098 4097 4096 4095 4093	8 58, 80-110 cm		28 57 56
F	Mixed middens			116-148 cm, floor		53 52 39 40
G	Older brown midden	19		5 9 15 36 35 34 33 32 31 30 28		45 60
H	Primary clays derived from local bedrock	18	4092	26 25		51 50

Table I.—*Relative stratigraphic relationships of samples of the six controlled pollen sequences. Extramural samples are assigned specimen numbers by location.*

Strati-Graphic Unit	Sample Number	Adj. AP %	CPPC Date AD	MCD Date AD	N of Decorated Sherds
A	27	X		1300 ± 0	1
D	26	54.4	1275-1240	1252 ± 47	24
	25	53.9			
E	24	63.8			
	10	77.0			
	23	64.3	1240-1215		0
	22	59.5			
	21	66.0			
	20	62.4			
	16	58.5			
G	19	53.9	?1215-1125		0
H	18	53.0	?		S

Table II.—*Relevant chronometric data for the test at 416.7N, North Pueblo. S = culturally sterile, X = pollen yield insufficient for analysis.*

Strati-Graphic Unit	Sample Number	Adj. AP %	CPPC Date AD	MCD Date AD	N of Decorated Sherds
A	4105	59.0	?		S
	4104	54.5			
B	4103	X	1465-1425		0
D	4102	53.5			
	4101	48.0			NA
	4100	64.0	1425-1335		
E	4099	42.0	1335-1315	1350 ± 0	1
	4098	35.0		1327 ± 64	11
	4097	18.0			
	4096	40.0	1315-1275	1333 ± 32.8	5
	4095	20.0		1168 ± 0	1
	4093	30.0	1275-1240	1335 ± 34.3	23
H	4092	X		1348 ± 35	4

Table III.—*Relevant chronometric data from the "ballcourt" backhoe trench 1, South Pueblo. Pollen data from the samples collected at locus 56N. Ceramic data from the arbitrary 10 cm levels of test pit at locus 39N correlated by natural stratigraphy. NA = stratigraphic unit not exposed at 39N.*

Strati-Graphic Unit	Sample Number	Adj. AP %	CPPC Date AD	MCD Date AD	N of Decorated Sherds
B				1298 ± 86	5
D	2	53.8	1275-1240	1244 ± 106	2
E	8	57.8		1213 ± 88	4

Table IV.—*Relevant chronometric data from the test of 22S, South Pueblo.*

behavior at Nuvakwewtaqa. For example, Adj. AP % values in the 22.5 to 58.5% range occur at all the test locations. Pollen values of this range are dated to the A.D. 1240-1275 episode by the CPPC. Though episodes characterized by the same values occur earlier (A.D. 1125-1215) and later (A.D. 1315-1335), A.D. 1240-1275 is the most probable chronometric alternative represented in all of the tests from the site. If the 1240-1275 antiquity estimate is correct, the CPPC predicts that directly superimposed pollen records which have Adj. AP % values outside the 22.5 to 58.5% range will have values less than 22.5% , and pollen records directly superimposed by those attributable to the 1240-1275 episode will have Adj. AP % values greater than 58.5%. These predictions are observed in each of the six test pit cases. Such consistency is not expected if the Adj. AP % value pollen statistic were a product of human behavior, considering the variety of activity areas represented by the test pit locations.

With that concern allayed, it is clear from the tables that the chronometric estimates of antiquity provided by the CPPC for the various samples do not correspond to the estimates of relative antiquity that would be assigned to them by virtue of their contextual association with a particular stratigraphic unit. Though the variously colored sheet middens of the site were always observed in the stratigraphic order identified for them, both the MCD and the CPPC dates suggest that their deposition was not temporally uniform throughout the site. Younger brown midden samples are dated palynologically to the A.D. 1425-1335 episode at the ballcourt, to the A.D. 1315-1275 episode at 33.8S, and to the A.D. 1275-1240 episode at the tests at 22S and 150S. Similarly, red-brown midden is variously dated A.D. 1335-1315, 1315-1275 and 1275-1240 at different locations. MCD dates for both midden types overlap throughout the 13th and 14th centuries, also. Taken in the context of Coinman's analysis of the vertical distributions of ceramic type clusters at the North Pueblo, these results confirm her conclusion that the evidence which is now available suggests necessity for an alternative to Upham's chronology.

Coinman's concern regarding generalization from the MCD chronometric results in the room at 33.8S to the whole of the site and the Chavez Pass District also is justified by the results of application of the CPPC. Even discounting those situations in which MCD dating is constrained by very low numbers of decorated potsherds in the assemblage, the correspondence between MCD dating and CPPC dating of potentially concordant records is poor. There are nine cases in which MCD dating is based upon five or more examples of decorated pottery in the ceramic assemblage. In six of those cases the standard deviation of the MCD date is so wide that another Adj. AP % value could also be considered valid, since the span of time expressed by one standard deviation on either side of the MCD encompasses effective moisture episodes of two or three forms. In one other case, the concordance expected if the assumption is valid is not observed. Thus only two of the nine cases yield concordant MCD and CPPC dates consistent with the assumption upon which Upham's model was constructed.

The results of this pollen analysis, however, do not positively disprove Upham's chronometry for the process of establishment, development and abandonment of the North Pueblo, Nuvakwewtaqa and the Chavez Pass District nor do they provide positive support for either alternative chronometry. Five samples of floor deposits from as many segregate rooms excavated or tested at North Pueblo in 1981 and 1982 were selected for study on the basis of their stratigraphic positions and the temporal ranges of the

Strati-Graphic Unit	Sample Series/ Depth	Adj. AP %	CPPC Date AD	MCD Date AD	N of Decorated Sherds
B	1/0 cm	14.3			
	2/0 cm	19.1	?1595-1575		
	2/10 cm	17.6			
	1/10 cm	55.0			
C	3/50 cm	43.3			
	3/60 cm	19.0			
	3/70 cm	46.1	1335-1315		
	3/80 cm	25.3			
	3/90 cm	X			
	3/100 cm	26.1			
				1323 ± 36	147
D	2/20 cm	15.2			
	2/30 cm	14.1			
	1/35 cm	X			
	2/40	28.8			
	2/50 cm	8.5			
	1/55 cm	X			
	1/58 cm*	97.0			
	2/60 cm	0.1	1315-1275		
	2/70 cm	X			
	1/90 cm	X			
E	2/80 cm	0.8			
	2/100 cm	X			
	1/100 cm	20.7		1285 ± 31	51
	2/110 cm	17.7			
F	1/116 cm	0.6			
	1/120 cm	19.6			
	2/120 cm	X			
	1/130 cm	X			
	2/130 cm	5.6		1157 ± 146	4
	1/135 cm	X			
	1/143 cm	X			
	2/ floor	X			
	1/ floor	28.8	1275-1240		

Table V.—*Relevant chronometric data from room at 33.8S, South Pueblo. * = lens of red-brown midden.*

Strati-Graphic Unit	Sample Number	Adj. AP %	CPPC Date Ad	MCD Date AD	N of Decorated Sherds
B	37	52.7	1335-1315	1297 ± 53	20
G	5	51.3			
	36	X	1275-1240		
	35	56.7			
	9	61.8			
	34	75.0	1240-1215		
	33	66.7		1315 ± 26	6
	32	X	1215-1125		
	31	52.7			
	30	15.7			
	28	X			
			1125-1075		
H	26	X			0
	25	48.8	?		

Table VI.—*Relevant chronometric data from the test at 121.3S, South Pueblo.*

artifacts associated with them. Two were of younger and three were of older room floors, as judged by those criteria. Only one of these samples contained sufficient pollen for analysis. Since potentially older or younger pollen records of room floor context are not available from the North Pueblo for comparison, and since no independent chronometric estimate of the antiquity of this sample is available which would allow its comparison with contemporary samples from other parts of the site or other sites, the operating assumption that its Adj. AP % value is culturally induced cannot be tested. The CPPC, then, cannot be applied to establish the antiquity of the single North Pueblo room floor pollen record.

However, this pollen sample yielded an Adj. AP % value of the type which has been dated A.D. 1340-1425 elsewhere at Nuvakwewtaqa, depending upon stratigraphic position. If we were simply to assert the hypothesis that this date was accurate, we might suppose the hypothesis was amenable to testing through the analysis of additional pollen samples—dozens if necessary. Given the potential affect of the test upon the credibility and continued use of Upham's processual model, such a test would seem both a significant and a pertinent application of the Colorado Plateau Pollen Chronology.

But what is actually at issue in the debate over the chronometry of events at Nuvakwewtaqa is the affect the chronometry has on Upham's processual model. The problem is not whether *no* occupation occurred at the North Pueblo after A.D. 1315. Rather, it is whether whatever degree of abandonment of the North Pueblo occurred in the 13th century reflects a pattern of widespread political and economic destabalization affecting other Puebloan communities at that time.

There are two reasons why the CPPC cannot be employed to produce significant amounts of information relevant to this critical issue. One is that it is not designed to do so. Application of the CPPC informs one of the antiquity and the effective moisture value at the time of the deposition of a pollen record drawn from a Colorado Plateau archaeological context dating within the past 2000 years. It is not designed to do anything

else. The problem of significant archaeological interest is neither a chronometric nor a paleoenvironment description problem, but rather one of behavioral reconstruction. Methods of pollen analysis exist that allow behavioral reconstructions, but the CPPC is not such a method.

The other reason is that study of additional room floor samples from North Pueblo is likely to be inconclusive even if the research could be framed in relevant chronometric or paleoenvironmental terms. It was noted earlier that almost a full fourth of the samples used in this pollen study did not yield sufficient pollen for analysis, and of the eight room floor samples studied from the site (two from 33.8S and six from the North Pueblo), 72.5% failed to yield sufficient pollen. There is no reason to assume that study of additional room floor context samples would prove more productive. Even if as many as 70% of additional room floor samples from the North Pueblo produced sufficient pollen for analysis, however, it would not constitute an adequately large fraction of the rooms *potentially* dating to the 15th century to allow a test of the question of whether or not the North Pueblo had been fully abandoned by that date.

Strati-Graphic Unit	Sample Number	Adj. AP %	CPPC Date AD	MCD Date AD	N of Decorated Sherds
D	13	52.5			
	59	35.2		1270 ± 98	4
	58	X			
E	28	55.6			
	57	X		1281 ± 39	5
	56	X			
			1275-1240		
F	53	X			
	52	X		1279 ± 0	1
	39	39.2			
	40	45.7			
G	45	50.0		1103 ± 0	1
	60	41.7		1350 ± 0	1
H	51	77.8	?		0
	50	55.9	?		

Table VII.—*Relevant chronometric data from the test at 150S, South Pueblo.*

SUMMARY

The Colorado Plateau Pollen Chronology (Schoenwetter 1970) provides opportunity for a palynological test of two assumptions critical to a processual model which accounts for settlement pattern events of the 13th and 14th centuries in that region. When applied to relevant pollen samples from Nuvakwewtaqa, the results support criticisms of Upham's chronometry suggested by Coinman (1984). It would appear that the chronometry of strata deposition is not uniform across the territory of the site, even though the sequence of strata deposition may be. It also would appear that the

chronometry of ceramic assembled-stratigraphic superposition relationships which occurs at the test Upham considered "characteristic" for the site (Upham 1982:169) is not sufficiently replicated to be generalizable to the site as a whole or the Chavez Pass District.

This outcome, however, does not provide positive support for either of two published alternatives to Upham's chronometry of settlement events at Nuvakwewtaqa. On the one hand, this occurs because the critical archaeological issue involved is not answerable through production of the sorts of information the CPPC method is designed to produce. On the other hand, it occurs because the nature of the archaeological context from which the relevant samples can be collected seems unusually pauperized as regards preserved pollen. Thus the method probably cannot generate enough usable information to resolve the issue of the date of North Pueblo abandonment at Nuvakwewtaqa.

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Folk medicines continue to pose fundamental questions for professionals and lay persons alike: Do they "work?" and *How* do they "work?" One approach to such issues, as in this collection of papers, is "to examine the medical practices of non-Western cultures in order to establish a scientific basis for the successes of these remedies" (p. vii). That is to say, apart from any psychotherapeutic aspects of illnesses and healing, the authors here seek to discover biochemical components of the plants (in these instances) found in ethnopharmacopoeias that might be expected to have curative effects on diseases.

The contributors, almost all of whom are biochemists or pharmacologists, represent 9 different countries, and they examine "the medicines of 11 different cultures" (p. vii). This last characterization implies both too much and too little. In all of the 14 papers, only selected examples are discussed (and sometimes only a single species, e.g., *Allium sativum* and *Panax ginseng)*, and the "cultures" included range from the specificity of Aztec and Zuni to such generalities as India, Fiji, Papua New Guinea, and China.

Given the publisher and authors, it should not be surprising that most of the papers consist of highly technical analyses of the chemical constituents of the plants examined, despite the editor's claim (p. vii) that they are written in a manner "to satisfy the curiosity of the lay reader with a modest chemistry background." The specialist with a *strong* "chemistry background" may find some new information here regarding particular alkaloids or species, but the rest of us can only conclude—as we suspected before—that some "folk medicines" have a "scientific basis" and some do not, at least when the question is framed in very narrow terms.

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SEEDS OF DISCONTENT: IMPLICATIONS OF A "POMPEII" ARCHAEOBOTANICAL ASSEMBLAGE FOR GRAND CANYON ANASAZI SUBSISTENCE MODELS

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ABSTRACT.—Models of Anasazi subsistence economies in the Grand Canyon region are summarized. These models are then evaluated with new archaeobotanical evidence from a completely excavated Pueblo II Anasazi site located south of Desert View. Results of palynological and flotation analyses indicate that Anasazi subsistence along the South Rim and in the Upper Basin to the south focused on the exploitation and use of undomesticated pinyon nuts and amaranth seeds. However, it is difficult to reconcile the archaeobotanical data with the settlement's inferred permanent mode of occupation. Several alternative explanations are discussed.

INTRODUCTION

As the scope of survey and excavation has expanded in recent years, Southwestern archaeologists have recovered relevant data to support increasingly detailed pictures of Anasazi subsistence economies (e.g., Gasser 1982; Ford 1984; Gumerman 1984). This is especially true in the Grand Canyon area (Fig. 1) where regional sample surveys, supplemented by excavations in certain cases, have been conducted along the North Rim, the South Rim, and within the Inner Canyon. These studies have revealed that variation in subsistence practices was related, not surprisingly, to where the Anasazi lived in the Grand Canyon (Euler 1967).

Recent excavation of a Pueblo II Anasazi site (AZ I:1:17 (ASM)—Site 17 hereinafter), located just south of Desert View (Fig. 1), disclosed a well-preserved assemblage of archaeobotanical remains. Because Site 17 had been consumed rapidly by a catastrophic fire (Sullivan 1986), it is convenient to refer to the site's contents as "Pompeii" assemblages since they represent inventories of plants and artifacts at the time the settlement was destroyed and abandoned (Schiffer 1985).

It is important to note that this is the only substantial archaeobotanical collection recovered systematically from an archaeological site in the vast area between the South Rim and the San Francisco Peaks near Flagstaff. Also, this was the first excavation of an Anasazi site on the South Rim since the Tusayan Ruin was dug in 1929 (Haury 1931). The purpose of this paper, then, is to examine how these new data affect current views of Anasazi subsistence in the Grand Canyon area.

GRAND CANYON ANASAZI SUBSISTENCE ECONOMIES

Reconstruction of Grand Canyon Anasazi subsistence has been attempted, interestingly enough, only relatively recently. Based on fieldwork and pollen analyses conducted in the late 1960s and early 1970s (no flotation procedures were used then),

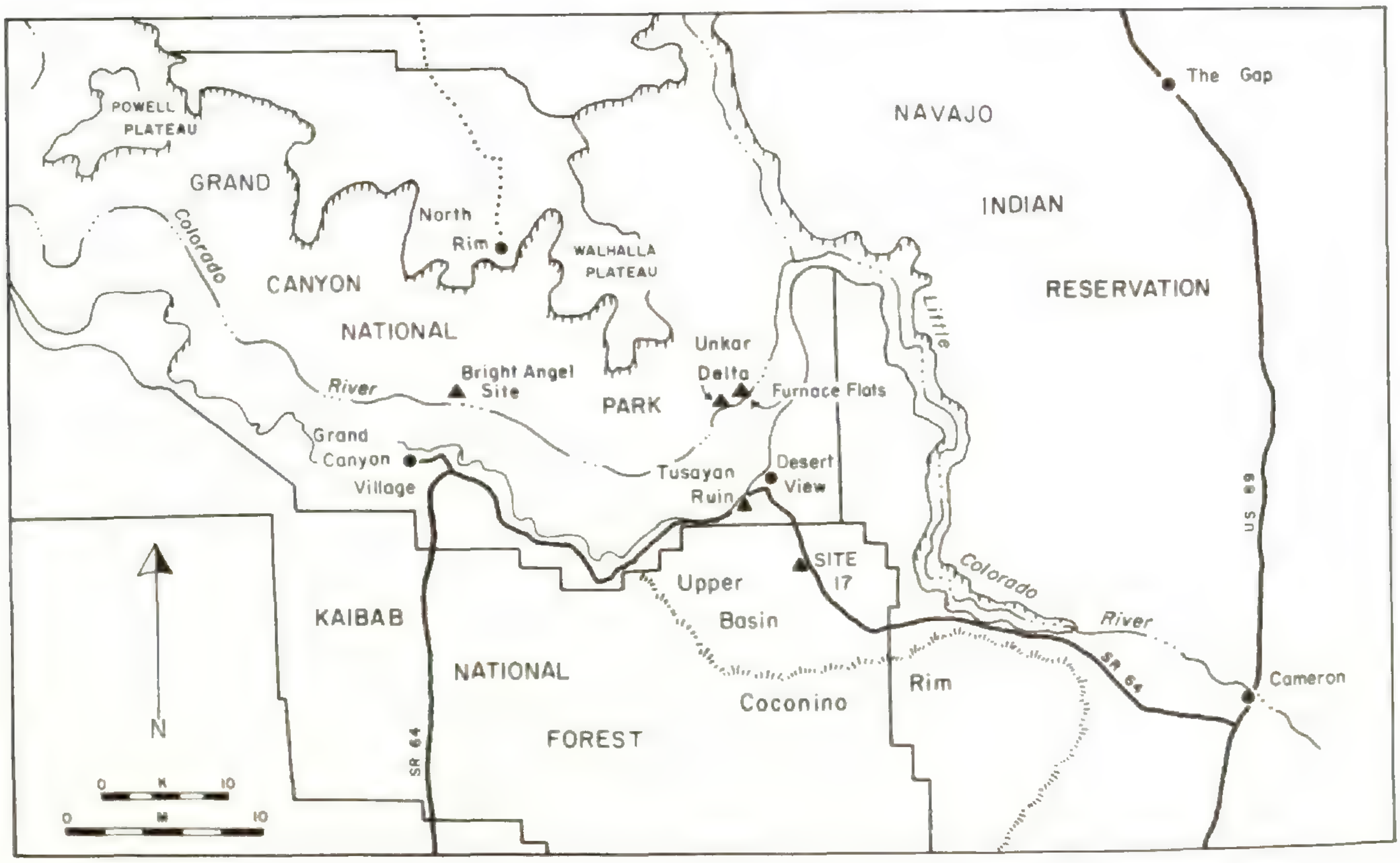

FIG. 1.—Important locations and archaeological sites in the Grand Canyon area.

Schwartz and his colleagues have posited that permanently occupied settlements (some with kivas—see Schwartz *et al.* 1979) along the Colorado River were linked with warm-season agricultural farmsteads located on the North Rim (Schwartz *et al.* 1981:6). In terms of economic organization, they argue that between A.D. 1050 and 1100, Anasazi groups were only "partially agricultural" (Schwartz *et al.* 1980:48, 175-176). Excavations at Unkar Delta (Fig. 1) produced small amounts of corn, bean, and squash pollen and relatively high frequencies of Mormon tea, cheno-am, and cactus pollen (Schwartz *et al.* 1980:179-180). Given the temperature and precipitation regimes of the Inner Canyon, it may have been possible to double-crop there (Jones 1986a:331).

Between A.D. 1100 and 1150, there appears to have been a substantial shift in economic organization. During this period of occupation on Unkar Delta, there is "indisputable evidence of agriculture" (Schwartz *et al.* 1980:185-187) in the form of terraces and check dams as well as corn, bean, and squash pollen from rooms and behind terrace alignments. However, no direct evidence was found regarding the use of wild plants (Schwartz *et al.* 1980:187).

On the North Rim, economic organization appears to have been less complex (Schwartz *et al.* 1981:39-40). Between A.D. 1050 and 1150, settlements "functioned as summer farmsteads used for temporary shelter and storage of crops by people whose winter homes were elsewhere" (Schwartz *et al.* 1981:57), presumably in the Inner Canyon (see also Jones 1986b:437).

Based on their surveys on Powell Plateau and along the South Rim, Effland *et al.* (1981) have reconstructed a rather different pattern of Anasazi resource exploitation. In view of the substantially lower frequencies of soil and water control features in these areas, Effland *et al.* (1981:41) concluded that "domesticates accounted for only a relatively small amount of the diet for these people. Products obtained from hunting and gathering provided the bulk of the subsistence base." However, the comparatively numerous one-room and two-room sites found in the Upper Basin, a subarea of the eastern South Rim (Fig. 1), led them to infer that it may have been a location of rather extensive agricultural production under the assumption that these small sites were the remains of field houses

(see also Rice *et al.* 1980:24). Effland *et al.* (1981:44) also argue that in order to exploit the rich floral and faunal resources of the South Rim, especially the eastern South Rim, "an annual seasonal settlement round" was necessary.

BACKGROUND FOR SUBSISTENCE: ENVIRONMENT AND RESOURCES OF THE EASTERN SOUTH RIM AND THE UPPER BASIN

The Upper Basin occupies the area between the South Rim of the Grand Canyon and the northern tip of the Coconino Plateau (Fig. 1; Strahler 1944; Babenroth and Strahler 1945). Elevation in the basin varies from 7200 feet (2195 m) at Desert View to 6000 feet (1829 m) at the base of the Coconino Rim. Today, the area receives between 14 and 18 inches (355-457 mm) of precipitation annually (Sellers and Hill 1972:240; also Metzger 1961). At least 148 days may elapse between frosts, which is substantially greater than the 101 days that were needed to grow corn on the North Rim (Schwartz *et al.* 1981:20; also Jones 1986b).

The vegetation of the eastern South Rim and the Upper Basin is dominated by a dense pinyon-juniper forest (Merkle 1952). Here, stone and wood for lithic technology and architecture were easily procured (Rand 1965; also Fogg 1965). Large and small game (especially deer, rabbits, and wood rats) were relatively abundant as were edible and nonedible plant products—pinyon nuts, juniper and amaranth seeds, pitch, yucca fiber, and sagebrush bark (see especially Schellbach 1933; McHenry 1934). Given the resources of the eastern South Rim and the Upper Basin, how do current subsistence models for the Grand Canyon Anasazi fare in view of excavated data from Site 17?

SAMPLING FOR SUBSISTENCE AT SITE 17

Excavations at Site 17 revealed the remains of a small Anasazi settlement (Fig. 2) that was occupied between A.D. 1049 and 1064 based on a tight clustering of 34 tree-ring dates (Fig. 3; Sullivan 1986:152-154). Architecturally, the site is composed of a semi-subterranean masonry structure (Structure 1) built in A.D. 1049 (remodelled in A.D. 1057) and three log-walled timber and brush structures (Structure 4 was built in A.D. 1056; Structures 2 and 3 were built in A.D. 1058; see Fig. 4). Extensive trenching and extramural stripping uncovered an area where ceramic vessels were made and fired (F-8 and F-38 in Fig. 2). No other exterior features that date to the main occupation of the site were found. As mentioned, during occupation this small settlement was destroyed catastrophically by fire. Fortunately, from an archaeobotanical point of view, the heavy roofs composed of timbers, poles, brush, and earth collapsed directly upon the occupied floors below, thereby producing a "Pompeii" assemblage of *in situ* botanical remains (Table 1) and artifact arrays.

All deposits, with the exception of backhoe trench spoil, were sifted through .25 inch (6.4 mm) mesh screens. This resulted in the recovery of three charred macrobotanical specimens from the floor of the masonry structure: two small 8-row corn cobs (Harinoso de Ocho: C. Miksicek, pers. comm.; cf. Cutler and Blake 1980) and a whorl of processed yucca *(Y. baccata)* fiber. All other archaeobotanical specimens were found in 17 flotation samples and 5 pollen samples taken from a variety of primary depositional contexts throughout the site (Fig. 2; cf. Toll 1984:244). Interestingly, no faunal remains that date to the main occupation of the settlement were recovered.

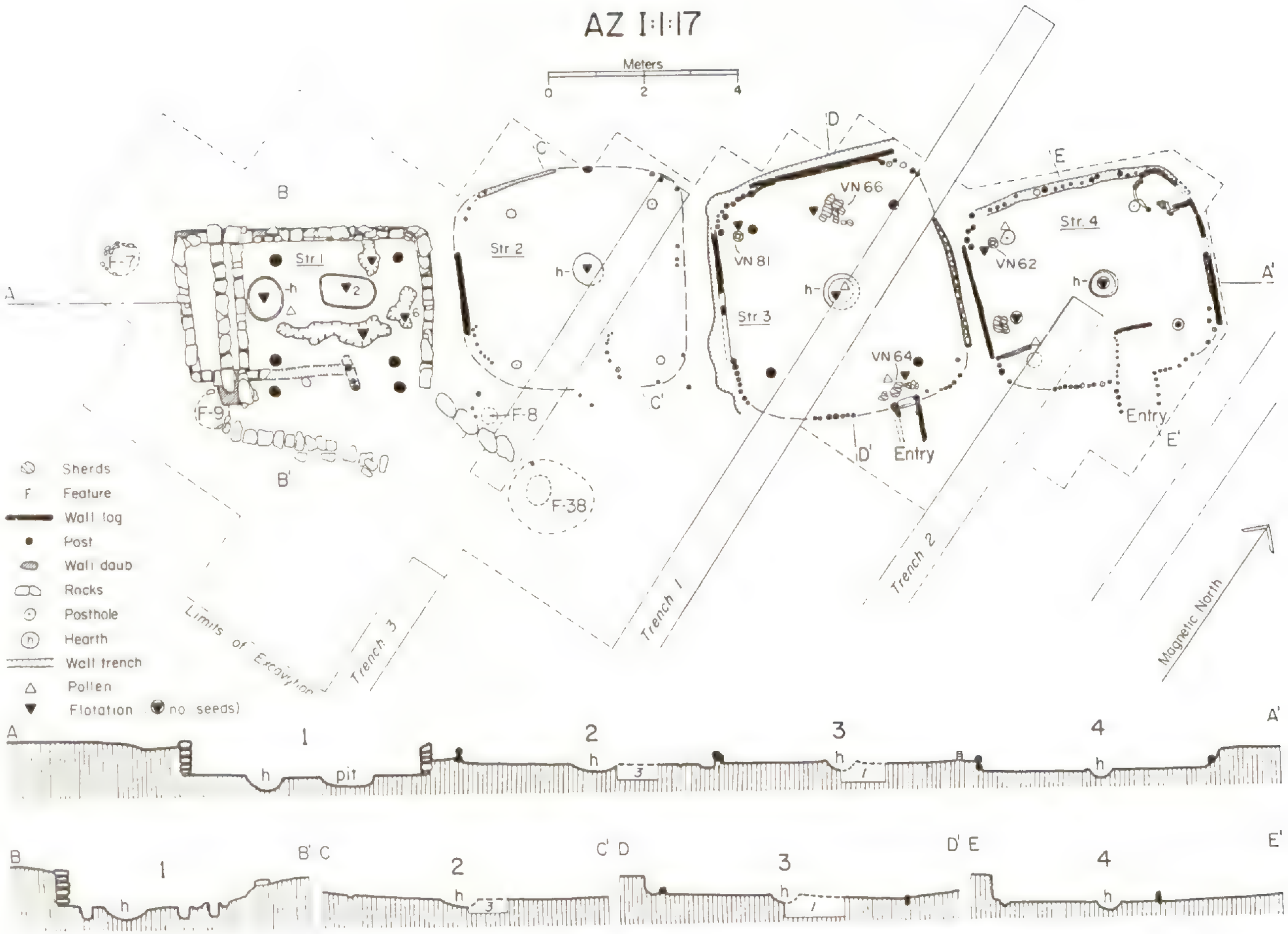

FIG. 2.—Map of Site 17 (AZ I:1:17 (ASM)) showing locations of pollen samples (open triangles) and flotation samples (filled triangles) plus other significant features of the site. The three ticket features in Structure 1 are unlined pits. The unticked feature in Structure 1 that is not labelled with a (h) is a clay-lined pit. All hearths (h) are clay-lined.

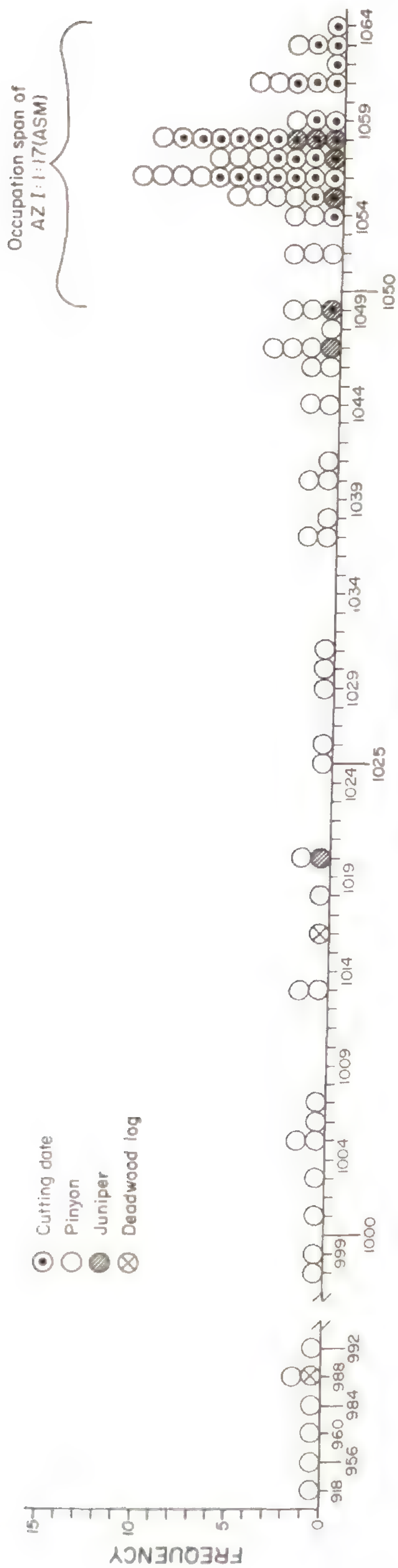

FIG. 3.—Histogram of cutting and noncutting tree-ring dates. Occupation span was determined by the range of cutting dates (A.D. 1049-1064).

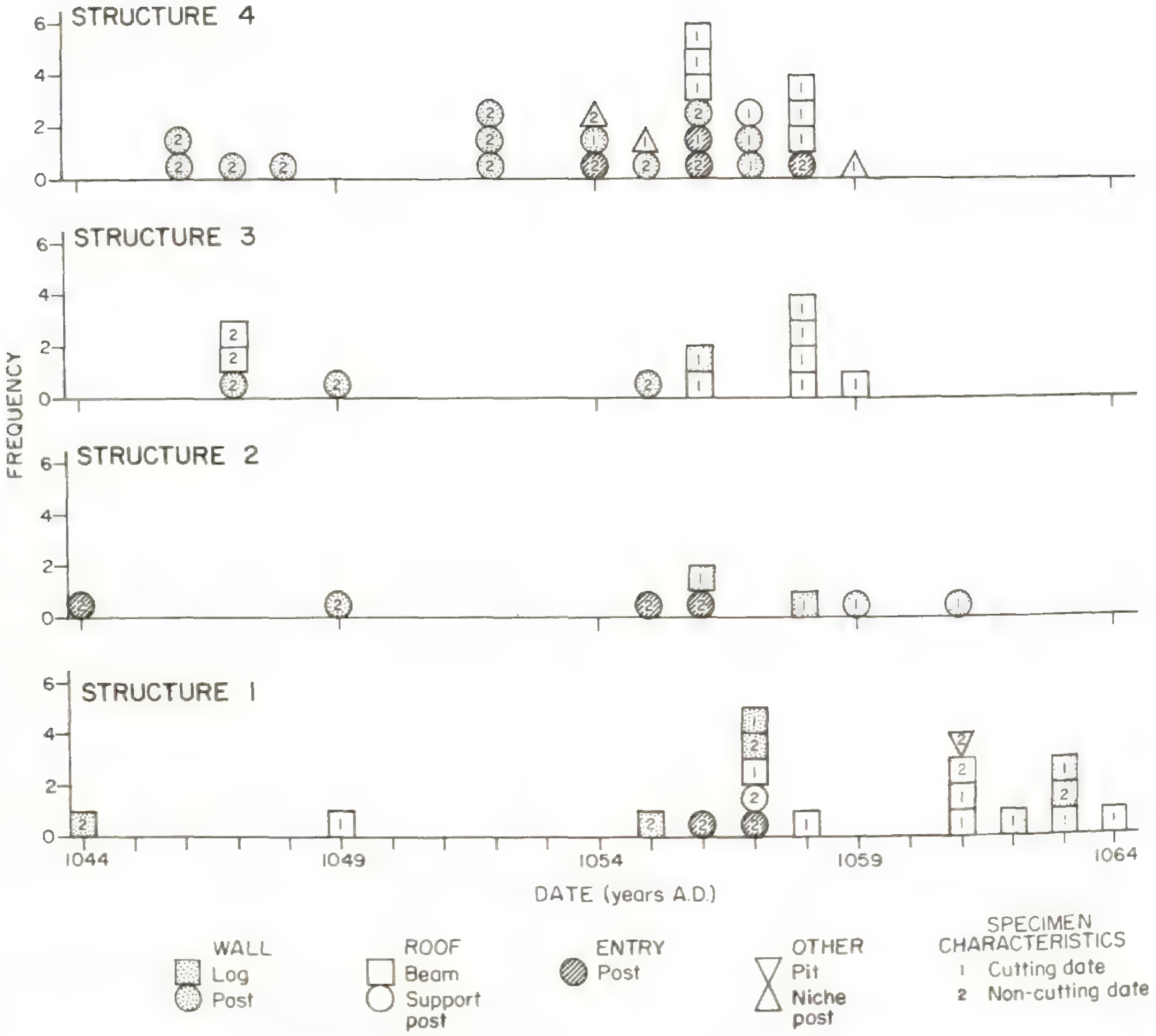

FIG. 4.—Histograms of cutting and noncutting tree-ring dates for all structures.

ARCHAEOBOTANICAL REMAINS FROM SITE 17

Palynological analysis revealed no domesticate pollen (Davis 1986:335). This is rather surprising since (a) five samples were from behavioral proveniences (one each from the floors of Structures 1 and 3, two from the floor of Structure 4, and one from the plastered hearth of Structure 3); (b) at least 200 grains were counted in each sample; and (c) as mentioned, corn macroremains had been recovered during excavation (cf. Bohrer 1981). These data suggest that domesticates, and corn in particular, were not processed in the settlement's structures (Davis 1986:335).

The composition of 17 flotation samples, broken down by taxon, structure, and provenience, is presented in Table 2 and represents a slight reworking of previously published data (for details on processing procedures, preservation, and contents of specific samples see Scott 1986). The information in Table 2 reflects the following collection and processing procedures. First, enough dirt was collected from the target provenience to fill one-half of a standard-sized #12 bag (ca. 4.7 liters). Second, depending on the size of the feature or vessel being sampled, the number of samples per provenience may have varied (see Table 2) as did, therefore, the quantity of soil floated (generally between 4

TABLE 1.—*Taxa identified in archaeobotanical samples from Site 17.*

TAXON	COMMON NAME
Amaranthus sp.	Amaranth (pigweed)
Chenopodium sp.	Chenopod (goosefoot)
Juniperus osteosperma (Torr.) Little	Juniper
LEGUMINOSEAE	Legume
Opuntia sp.	Cactus (prickly pear)
GRAMINEAE*	Grass
Pinus edulis Engelm.	Pinyon
Portulaca sp.	Purslane
Trifolium sp.**	Clover
Vitis sp.**	Grape
Zea mays	Corn

*Includes one *Oryzopsis* sp. seed and two unidentified grass seeds.
**Represented by a single seed (not included in subsequent analyses).

liters and 10 liters; Scott 1986:339). And third, due to the large volume of floated material "a minimum of 25 percent of each light fraction was examined" (Scott 1986:339).

In order to develop settlement-wide economic interpretations of the data in Table 2, the following strategy was adopted. The 17 samples were separated into two major classes: (a) all floor features (i.e., unlined pits, clay-lined pits, and hearths) and floor contact vessels and (b) hearths and floor contact vessels only. By restricting analysis only to floor contact samples, the possibility of including material that originated from architectural debris and post-occupational processes was reduced considerably (Doebley 1981:182). Also, I assumed that the contents of all floor features and vessels might include the plants or plant parts that had been processed and stored prior to the destruction of the settlement as well as any low frequency residuals that might be indicative of long-term plant use (Minnis 1981:145). Similarly, by examining the contents of hearths and vessels only, it seemed reasonable to expect that information regarding patterns of daily plant use might be revealed (cf. Hally 1981:724).

The data in Table 2 were also broken down in terms of (a) all plant forms (i.e., charred and uncharred seeds and seed fragments, twigs, and needles) and (b) charred seeds and seed fragments only (Toll 1984:245). The purpose of this aspect of the analysis was to segregate those botanical remains that may have been food—seeds and seed fragments—from those that represented other types of plant use (Minnis 1981:147). For example, the juniper twigs and pinyon needles recovered from the hearths of Structures 1, 2, and 3 (see Table 2) undoubtedly are the remains of fuel or fire-starting tinder (Madsen 1986:31; also Phillips 1909:220). The charred and uncharred needles from the clay-lined and unlined pits in Structure 1 may have been roofing material or boughs used for bedding (Adams 1980:59-61; also McHenry 1934:265), while the 100 + charred needles from a small Black Mesa Black-on-white pitcher (VN 81) on the floor of Structure 3 most likely are the residue from a medicinal decoction (Lanner 1981:62; also Schellbach 1933:224). In contrast, the hundreds of pinyon nuts and fragments recovered from a broken jar on the floor of Structure 3 (VN 66) and an intact jar on the floor of Structure 4 (VN 62) clearly represent stored food (cf. Lanner 1981:71; Madsen 1986:31-34).

Table 2.—*Archaeobotanical specimens (seeds unless noted otherwise) recovered from floor pits, hearths, and vessels at Site 17 (from Scott 1986:342).*

	Structure 1			Structure 2			Structure 3			Structure 4	
	Lined Pit (n = 2)	Unlined Pits (n = 8)		Hearth	Hearth	Hearth	VN64	VN66	VN81	VN62	
	Charred	Charred	Uncharred	Charred	Charred	Charred	Charred	Charred	Charred	Charred	TOTAL
JUNIPER											
Seeds/fragments	6	2		3	2		1				14
Twigs	4	11	6	3							24
PINYON											
Seeds/fragments		4		2	1		1	100 +[b]		200 +[c]	308 +
Needles	40	62[a]		100 +	14	7	11		100 +		334 +
AMARANTH	48	34	1	250 +							333 +
CHENOPOD	22	90		10							122
LEGUME	1	8									9
CACTUS								2		2	4
GRASS		2	1								3
PURSLANE	1	1		1							3
CORN											
Kernels/fragments										4	4
Cob fragments				2							2
TOTAL	122	214	8	371 +	17	7	13	102 +	100 +	206 +	1160 +

a = includes 5 twigs; b = seeds only; c = seed fragments only; n = number of samples; VN = vessel number

ASPECTS OF ARCHAEOBOTANICAL ASSEMBLAGE QUANTIFICATION

Describing variability among archaeobotanical samples is a matter of considerable methodological discussion. Two measures dominate the literature. Relative abundance, which is the proportion of seeds of a specific taxon in a given assemblage, is frequently used as a gauge of prehistoric floral importance (Miksicek 1983). According to Minnis (1986:209), though, relative abundance monitors the magnitudes of past accidents of preservation. Thus, a small number of spills can seriously bias the representation of individual taxa in macrofloral assemblages (also Adams 1980:9). With regard to the data from Site 17, the "bias by accident" problem is controlled to a large extent because nuts stored in vessels are obviously not accidents of preservation (cf. Hally 1981:729-730). One problem with relative abundance, however, is that taxa proportions are not independent on one another, i.e., the proportion of a specific taxon is dependent on the proportions of others (Miksicek 1983:679-680). More importantly, perhaps, relative abundance does not take into account the spatial distribution of a particular taxon because samples are lumped when proportions are calculated.

Ubiquity, which refers to the percentage of samples that produced specimens of a specific taxon, "tends to measure the number of accidents that occurred, which may in turn be a measure, albeit imprecise, of the degree of processing and consequently of the use of that taxon" (Minnis 1986:210). However, ubiquity values may overrepresent specific taxa because the occurrence of a single seed or several hundred seeds in a given sample are analytically equivalent (Miksicek 1986). Thus, while ubiquity controls for spatial variation among macrofloral samples, it exaggerates differences in content diversity by inflating the importance of uncommon types (Minnis 1980:380).

Relative abundance and ubiquity measure different aspects of archaeobotanical assemblage variability. Fortunately, the advantages and disadvantages of each counterbalance one another. For this reason, relative abundance and ubiquity values were calculated for both groups of samples and types of plant remains.

PATTERNS OF PLANT USE AT SITE 17

When all floor features and vessels are considered (Table 3 and Fig. 5), it is clear that pinyon is relatively abundant and ubiquitous (cf. Gasser 1981:312). Furthermore, charred pinyon, amaranth, and chenopod seeds are all moderately abundant and ubiquitous suggesting that they were major components of the diet. It also is evident that corn remains were neither abundant nor ubiquitous (cf. Doebley 1981; Minnis 1986:212); neither were cactus, grass, purslane, and charred juniper seeds (cf. Bohrer 1973; Adams 1980:21-31). Thus, it is likely that this latter set of archaeobotanical remains, including perhaps the corn as well, may represent the remnants of an earlier plant use pattern (see Hally 1981:737-740).

This picture is refined by focusing only on the relative abundance and ubiquity of plant remains recovered from hearths and floor contact ceramics (Table 4). Because these samples are *in situ* materials (Miksicek 1986) from storage vessels (VNs 62, 64, 66, and 81) and hearths (in Structures 1, 2, and 3), they are a snapshot of the taxa that had been harvested and processed just prior to the destruction of Site 17. Based on the data depicted in Fig. 6, only pinyon, charred pinyon nuts, and charred amaranth seeds are relatively abundant; pinyon remains and charred pinyon nuts also are very ubiquitous whereas charred amaranth seeds are not (cf. Jones 1986c:267). These data are strong evidence for the proposition that pinyon nuts and, to a lesser extent, amaranth seeds, were the primary dietary staples when the settlement was consumed by fire (cf. Ford 1984:131).

TABLE 3.—*Relative abundance and ubiquity of taxa recovered from all floor features and vessels at Site 17 (N = 1160 specimens from 17 samples).*

TAXON	RELATIVE ABUNDANCE		UBIQUITY	
	All Parts	Charred Seeds and Fragments	All Parts	Charred Seeds and Fragments
Juniper	3.3	1.2	64.7	29.4
Pinyon	55.3	26.6	100.0	47.1
Amaranth	28.7	28.6	58.8	58.8
Chenopod	10.5	10.5	58.8	58.8
Legume	0.8	0.8	17.6	17.6
Cactus	0.3	0.3	11.8	11.8
Grass	0.3	0.2	17.6	11.8
Purslane	0.3	0.3	17.6	17.6
Corn	0.5	0.3	11.8	11.8

In addition, there may have been a slight difference in how these two taxa were treated. Because pinyon nuts are ubiquitous, relatively abundant, and occur in a variety of contexts and forms, it is likely that they were being processed, stored, and (in some cases) consumed throughout the settlement (Madsen 1986:31-34). On the other hand, the relatively abundant yet concentrated amaranth seeds in the hearth of Structure 1

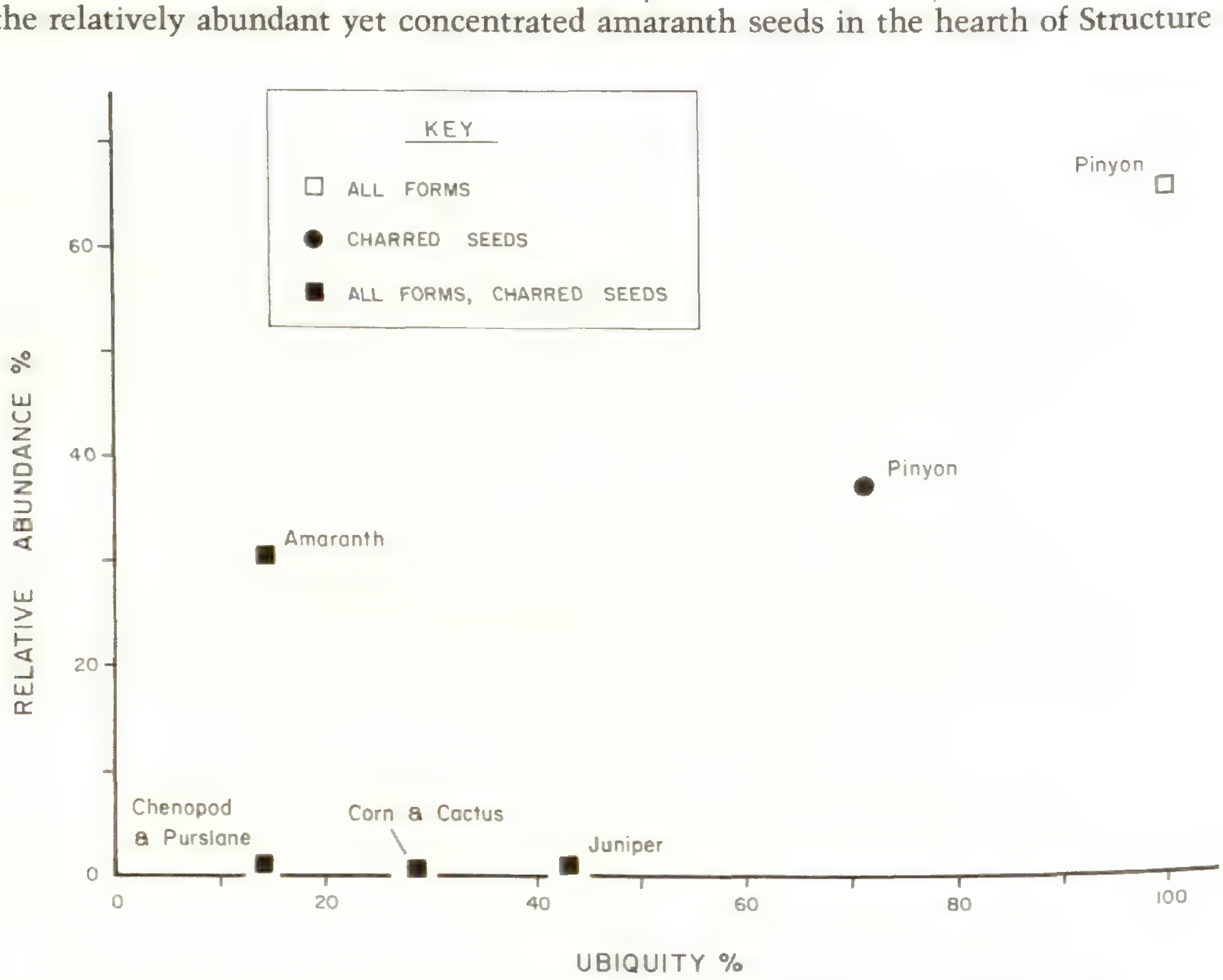

FIG. 5.—Plot of ubiquity and relative abundance values for all taxa recovered from all floor features (unlined pits, clay-lined pits, and hearths) and floor contact vessels.

may have been cooked as soon as they had been gathered and brought back to the settlement, rather than processed and stored for consumption at a later date (Adams 1980:59-60).

One clear fact emerges from these analyses. There is no way the data can be manipulated that would warrant any conclusion other than at the time of the settlement's destruction, its inhabitants were basically consumers of undomesticated seeds and nuts (primarily pinyon). Although corn kernels and cobs were found, their contexts of recovery and their forms (see Table 2) suggest that the consumption of maize was an inconsequential aspect of this Anasazi household's diet (cf. Gumerman 1984:84-85).

TABLE 4.—*Relative abundance and ubiquity of taxa recovered from hearths and vessels at Site 17 (N = 816 specimens from 7 samples).*

TAXON	RELATIVE ABUNDANCE		UBIQUITY	
	All Parts	Charred Seeds and Fragments	All Parts	Charred Seeds and Fragments
Juniper	1.1	0.7	42.9	42.9
Pinyon	65.7	37.3	100.0	71.4
Amaranth	30.6	30.6	14.3	14.3
Chenopod	1.2	1.2	14.3	14.3
Legume	0.0	0.0	0.0	0.0
Cactus	0.5	0.5	28.6	28.6
Grass	0.0	0.0	0.0	0.0
Purslane	0.1	0.1	14.3	14.3
Corn	0.7	0.5	28.6	28.6

"POMPEII" ARCHAEOBOTANICAL ASSEMBLAGES: HOW REPRESENTATIVE?

The conclusion that the occupants of Site 17 were seed and nut eaters rather than corn consumers is based on a well-preserved archaeobotanical assemblage from a tightly-dated site. This "Pompeii" assemblage, however, represents the composition of the household larder on the day the settlement burned and thus presents two interpretive possibilities depending on how its representativeness is viewed (Hally 1981).

The assemblage from Site 17 is quite different than that of an Anasazi site—Furnace Flats—located in the eastern Inner Canyon (Jones 1986a; see Fig. 1). High ubiquity of corn remains and cheno-am seeds from Furnace Flats led the investigator to conclude that this small Pueblo II site was occupied during the late summer and that the "cultivation of corn provided a good portion of the total foodstuffs but was supplemented by wild and gathered foods, such as tree legumes, cacti, and chenopods" (Hutira 1986:289). In view of the Furnace Flats evidence, the assemblage from Site 17 may reflect plant use patterns associated with corn crop failure (cf. Bohrer 1975). This inference is based on the assumption that these people were maize agriculturalists (cf. Wagner *et al.* 1984:616) whose food production system had proved unreliable, thus necessitating extensive use of undomesticated plants (Doebley 1984:52). The corn kernels and cobs recovered at Site 17, then, may have come from previous corn crops that had been successfully grown and harvested, albeit at some distance from the settlement.

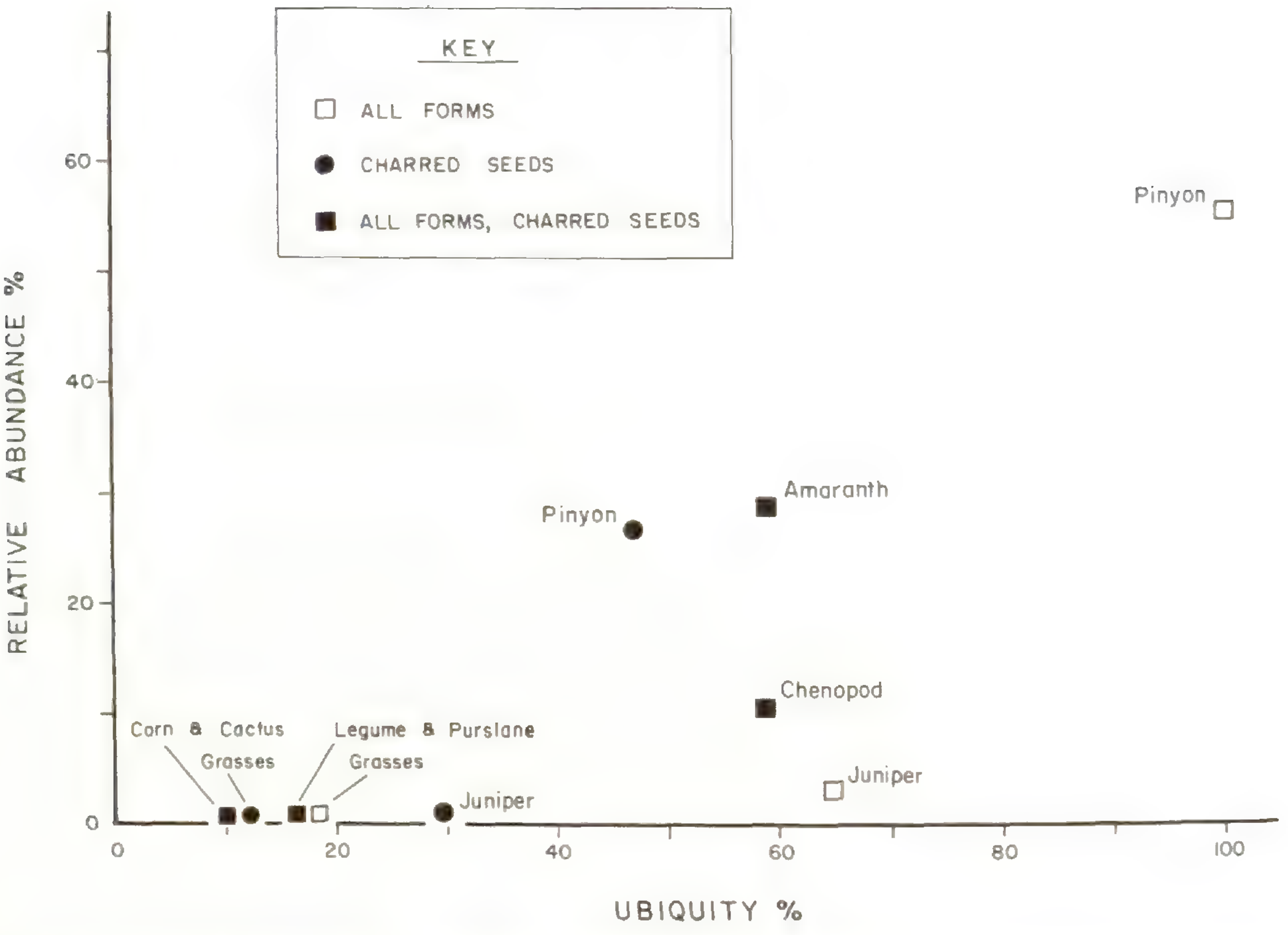

FIG. 6.—Plot of ubiquity and relative abundance values for all taxa recovered from hearths and floor contact vessels.

The other alternative is that corn agriculture was never a major component of Anasazi subsistence along the eastern South Rim and, therefore, the floral assemblage from Site 17 is representative of the yearly plant use pattern (see Gasser 1982:46 ff.). A consideration of the availability of corn, amaranth, and pinyon provides some support for this possibility. As a warm season annual, amaranth may be procured between June and September (Bye 1981:110-114; also Adams 1980:21). Pinyon nuts, however, are not ready for harvesting until the beginning of October (Martin 1973:1450; Madsen 1986:29). In terms of scheduling, therefore, the harvesting periods of amaranth and pinyon do not overlap. Each, however, overlaps with corn which is harvestable in late summer and early fall (Minnis 1985:316, 330). If corn had been available, then it is reasonable to expect that some of it would have been collected along with the amaranth (or vice versa—see especially Winter and Hogan 1986:119-121, 123, 138). Thus, the absence of corn pollen very well may mean that corn was not grown in the vicinity of the settlement and that, in addition, even if corn had been introduced to the settlement in a processed form, it was never an important element of the diet (cf. Gasser 1981:312).

SEEDS OF DISCONTENT: DISCUSSION

The extensive collection of undomesticated plant remains recovered from Site 17 presents a problem in archaeobotanical interpretation. Based on current thinking about Anasazi subsistence (e.g., Gasser 1982; Gumerman 1984; Wagner *et al.* 1984), it is difficult to reconcile the seed and nut remains with other aspects of Site 17. For example, the following characteristics suggest a perennial (e.g., Schwartz *et al.* 1980:67-74;

also Gumerman 1984:90) rather than a seasonal (e.g., Schwartz *et al.* 1981:45, 52-57; also Epstein and Schwartz 1951:83-85) occupational mode:

(a) annual repair and rebuilding events (based on clusters of tree-ring dates; see Fig. 4);

(b) summer tree-cutting activity (all tree-ring samples, with three exceptions, have incomplete terminal growth rings: J. S. Dean, pers. comm.);

(c) bedrock floors (Structures 1 and 4) and plaster floors (Structures 2 and 3);

(d) a wide range of moderately large-sized rooms (11.5 - 22.6 square meters);

(e) postholes sunk 35-50 cm into bedrock;

(f) clay-lined hearths in each structure; and

(g) functionally-specific room (grinding stones were found only in Structure 4; Structure 1 was probably a foul-weather living room (its semi-subterranean walls were clay-lined) and Structure 3 a fair-weather living room).

Thus, if Site 17 represents the remains of a permanently occupied habitation site, then (a) subsistence was based primarily on nuts and seeds (e.g., Martin 1973:1449-1450; also Madsen 1982) or (b) nuts and seeds were simply backup foods when domesticates were unavailable (Lentz 1984:198-199). Unfortunately, even though the entire site was excavated and all contents screened, there are presently no available archaeological or archaeobotanical data that can eliminate either possibility. Clearly, the near absence of corn, a domesticated dietary staple that generally is associated with habitation sites in the Grand Canyon area and elsewhere throughout Anasazi country (e.g., Toll 1984), is most unexpected.

ANASAZI SUBSISTENCE ECONOMY IN THE GRAND CANYON AREA: SUMMARY AND CONCLUSIONS

A reanalysis of archaeobotanical data from Site 17 provides a foundation for evaluating current subsistence economy models for the Grand Canyon Anasazi. First, many of the one-room archaeological sites—which Site 17 appeared to be prior to excavation—along the South Rim and in the Upper Basin may not have been related to agricultural production. Thus, previous archaeological surveys that interpreted these sites as the remains of fieldhouses may have exaggerated the importance of prehistoric agriculture in the area. Based on current evidence, farming in the Inner Canyon and on the North Rim probably exceeded that of the eastern South Rim and the Upper Basin (cf. Effland *et al.* 1981:40).

Second, it appears that small, well-built settlements may have been occupied (for various spans of time) by autonomous households that made pottery and subsisted mainly on undomesticated nuts and seeds (cf. Rice *et al.* 1980:58). This rather unconventional interpretation of an Anasazi habitation site (cf. Gumerman 1984:142-144) implies that, overall, there was substantially more variation in land-use, settlement, and subsistence patterns along the South Rim and in the Upper Basin than previous workers have suggested on the basis of survey data alone (e.g., Effland *et al.* 1981; also Euler and Chandler 1978).

Finally, there appear to be striking differences between the North Rim and the South Rim in terms of economic organization. The economic pattern of the North Rim/Inner Canyon Anasazi can best be described as a system structured on both delayed returns (e.g., food production and environmental manipulation) and immediate returns (e.g., collecting and encounter hunting—Woodburn 1980:98-99), although given the evidence

presented above, there may have been somewhat more of an emphasis on delayed or indirect resource procurement (see Bailey 1981:7). Furthermore, the presence of kivas at many Inner Canyon sites may indicate a concern with activity scheduling and with past and future events, features often associated with societies whose economic systems are not based exclusively on immediate returns (Woodburn 1980:97-98, 107).

In contrast, Anasazi economic organization on the South Rim and in the Upper Basin was based largely on a system of immediate returns through direct resource exploitation. Such patterns are especially useful in broad spectrum environments like the Upper Basin (Bailey 1981:6). The lack of evidence of kivas along the South Rim (Rice *et al.* 1980:22), until late in the occupational sequence (Haury 1931; Bannister *et al.* 1968:11), fits well with Woodburn's immediate return model.

A major lesson from Site 17 is that the emerging picture of robust variation in Grand Canyon Anasazi subsistence practices must be balanced by a consideration of the many factors—cultural and noncultural—that affect the composition of archaeobotanical assemblages (Bohrer 1986:36-38). For the future, archaeological research in the Grand Canyon area (cf. Euler 1974; also Schwartz *et al.* 1980:177) must concentrate on obtaining comparative botanical material from excavated sites. It is crucial to determine, for example, whether the "Pompeii" assemblage from Site 17 is simply a well-preserved, though seasonally skewed, archaeobotanical sample that reflects autumnal plant use strategies necessitated by an inadequate corn harvest (or perhaps outright crop failure). If this possibility can be eliminated by the excavation of sites that date to the same occupational horizon, then the South Rim and the Upper Basin may have been occupied by Anasazi who survived, in fact, largely by eating undomesticated nuts and seeds.

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Oriental Materia Medica: A Concise Guide. Hong-yen Hsu, Yuh-pan Chen, Shuenn-Jyi Shen, Chau-shin Hsu, Chien-Chih Chen, and Hsien-Chang Chang. Long Beach, CA: Oriental Healing Arts Institute, 1945 Palo Verde Ave., Suite 208. n.d. Pp. xxx + 933, line drawings. $59.95.

The Oriental Healing Arts Institute has produced a number of interesting books but none so outstanding as this volume. It is the product of six distinguished scholars, all of whom are pharmaceutical and chemical scientists from Japan and Taiwan.

Most of the entries concern plants, and a total of 768 species and varieties are considered: their technical, English, and Chinese names; origins; essences and flavors; traditional uses; and chemical constituents, pharmacology and dosage are outlined, including the molecular structures of many of the lesser-known chemical constituents.

The book is an encyclopedic compendium of "knowledge accumulated over two millennia" and presents the uses of the plants in traditional medicine as well as the latest available scientific information. The vast amount of material is made easily accessible by the five indices: common names in English; Chinese names in Pin-Yin transliteration; Chinese names in Wade-Giles transliteration; scientific and pharmaceutical names; and Japanese names in Hevon transliteration. There are several additional lists of great help to the reader: references (27 items), register of Pen Tsao literature (75 items), and a glossary of Western names of diseases.

This is a volume the utility of which goes far beyond oriental delimitations. It should be on the shelf of every library dedicated to the ethnopharmacological study of plants. The authors and the Oriental Healing Arts Institute must be congratulated for this superb contribution.

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TAPHONOMY AND ARCHAEOLOGICALLY RECOVERED MAMMAL BONE FROM SOUTHEAST MISSOURI

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ABSTRACT.—A number of nonhuman predators have been identified by zooarchaeologists and paleontologists as potentially contributing to, or otherwise affecting, the pre-depositional histories of faunal assemblages. Studies of modern bone destruction and modification resulting from feeding activities of gray wolves, and analysis of preferred prey species of owls are applied to identified mammal bone from Granite Quarry Cave in Southeast Missouri. Close similarity between mammal remains resulting from wolf and owl predation and archaeologically recovered remains from Granite Quarry Cave indicates that nonhuman agents were responsible for a large proportion of the archaeological faunal assemblage.

INTRODUCTION

A number of taphonomic agents have been identified by zooarchaeologists and paleontologists as potentially contributing to, or otherwise affecting, the pre-depositional histories of faunal assemblages (Andrews and Evans 1983; Blumenschine 1986; Brain 1981; Hill 1979; Levinson 1982; Potts 1984; Mayhew 1977). Recent studies of nonhuman bone accumulating and modifying agents in North America include, among others, activities of carnivorous predators such as wolves, coyotes, foxes, badgers, and cats, and raptors such as owls and hawks (e.g., Bickart 1984; Binford 1981; Bonnichsen 1973; Dodson and Wexlar 1979; Haynes 1982; Korth 1979; Kusmer 1986; Mellett 1974). Of these animals, two groups of predators, the canids (particularly wolves) and raptors (owls) are now, or have been in the relatively recent past, widespread across the North American continent (Bent 1938; Hall 1981).

Beginning in 1986 studies were conducted by the authors on bone modification by gray wolves (Snyder and Klippel 1986) and on preferred prey of owls inhabiting the southeastern United States (Klippel and Parmalee 1986). Recent identification and description (Klippel *et al.* 1986) of an archaeological vertebrate assemblage from Granite Quarry Cave (23CT36), located on the Current River in Southeast Missouri, provided an opportunity to apply information gathered through actualistic studies to the interpretation of an archaeologically recovered faunal assemblage.

THE ARCHAEOLOGICAL SITE

Granite Quarry Cave (23CT36) is a west-facing cave situated high on a limestone bluff along the left bank of the Current River in Carter County, Missouri. The cave entrance is approximately 9.5 m wide and 3.5 m high at the dripline, and the entrance passage extends roughly 30 m back into the bluff before opening into a larger room.

Archaeological testing by James Price in 1985 revealed culture-bearing deposits extending ca. 15 m from the dripline into the cave and to a maximum depth of approximately 60 cm below the surface (Price 1986). Three one-meter square test units, plus a portion of a fourth, were excavated and deposits screened through 6.4 mm (¼-inch) mesh hardware cloth. Price identified five strata within the cave deposits: predominately Historic, Mississippian, two underlying nonceramic zones and a basal stratum lacking cultural materials. Archaeological materials recovered during testing included 53 shell tempered sherds, 23 lithic artifacts, 2 modified bone artifacts, and 2 modified shell artifacts; debitage included ca. 2,500 chert flakes, 825 shells and shell fragments (the majority of which were from terrestrial species), and roughly 7,000 bones and bone fragments. Detailed descriptions of these materials are provided by Price (1986) and Klippel *et al.* (1986).

CANID FEEDING BEHAVIOR

Carnivorous predators may leave a residue of broken and scattered bone in a variety of settings such as kill sites, denning areas, or temporary resting and gathering spots (Binford 1981; Brain 1981; Sutcliffe 1970). When these areas are also the locus of human activities the resultant deposit will be a mixture reflecting both the human and nonhuman contributors. In order to recognize the contributions of nonhuman predators, it is necessary to isolate and characterize residues and damage patterns produced by the carnivores themselves.

In North America, the effects of wild predators on prey species have been studied by both wildlife biologists interested in predator prey relations (e.g., Floyd *et al.* 1979; Mech 1966, 1970; Pimlott *et al.* 1969) and by paleontologists and archaeologists interested in assessing the contributions of various taphonomic agents to bone accumulations (e.g., Mellett 1974; Haynes 1981, 1982, 1983).

Haynes, in particular, has studied a wide range of predators; he has collected information from kill sites, denning areas, and controlled feeding of zoo animals. Results of these studies have been influenced by both the varied activities of free ranging animals in the wild and by the artificial surroundings and feeding limitations often placed on zoo populations.

Control data.—Our study focusses on a single predator, the gray wolf *(Canis lupus)*, once common throughout much of North America (Hall 1981; Young and Goldman 1944), and a single prey species, the white-tailed deer *(Odocoileus virginianus)*, also common throughout much of eastern North America prehistorically. Circumstances of the study allowed the animals unrestricted access to "prey" carcasses, and controlled recovery of nearly all bone including remains from feeding activity and scatological materials.

In this study one adult female and four adult male wolves were allowed to range freely in a tree filled enclosure measuring approximately 38 m by 15 m. Three of the males were 3.5 year-old litter mates weighing between 45 and 50 kg. The fourth male was seven years old and weighed about 49 kg; the 2.5 year-old female weighed approximately 32 kg.

The wolves have been kept in this enclosure since April 1979, which contains an artificial den constructed of metal drainage pipes covered with earth and rocks. Since there is no ground cover, surface visibility is excellent. The usual diet of the wolves consists of commercially prepared dry dog food fed daily and one chicken per wolf, once a week. In addition, they are occasionally fed portions of deer or beef carcasses donated by local hunters and farmers.

Between March and November (1986) fresh, undressed carcasses of road-killed deer were placed within the enclosure at periodic intervals as they became available. Before a fresh carcass was introduced the entire pen area was surveyed, and all bone debris removed. While a carcass was being monitored the wolves received no other food.

Over the course of the study a consistent pattern of carcass reduction and disarticulation was observed. In the first 24 to 48 hours major meat sections such as the hindquarters were consumed, the thoracic cavity opened and ribs eaten away. The throat was also sometimes opened, and the nose was often eaten away. Within 48 hours disarticulation of one or more limbs occurred, usually forelimb before hind limb. Following consumption of meaty parts there was extensive destruction of limb bones, the vertebral column and associated rib heads and proximal shafts. In nearly all cases, remnants of the vertebral column and hide were the last carcass portions consumed. When all meat was consumed and the wolves showed no further interest in the remains (usually 4 to 7 days), the surface of the enclosure was surveyed and all remaining bone and bone fragments collected. Elements recovered from four deer carcasses are summarized in Table 1.

Of the skull and axial skeleton, tooth rows—both maxillary and mandibular—survived well, as did innominates. All left and right mandibles were represented, although the degree of destruction to individual elements varied. Seven of eight innominates also survived in identifiable form, most commonly the acetabula with broken ischial, pubic or ilial wings. Conversely, only one partially complete cranium was recovered, and vertebrae were poorly represented. Only three cervical vertebrae, exclusive of the atlases, and two of 52 thoracic vertebrae were recovered.

Of the appendicular skeletal units, the sturdiness of two joints, the distal humerus-proximal radius/ulna and distal tibia-proximal metatarsal, were exhibited in several instances in which these elements were found still firmly articulated and bound by dried bits of muscle and ligament. No distal radii or ulnae survived and only two of 48 carpals were collected. Many of the larger fibular tarsals and tibial tarsals, and seven of eight proximal metatarsals were recovered. Metacarpal and metatarsal shafts, still encased in hide, were also common. Finally, of 96 first, second, and third phalanges, only one was recovered.

In addition to remaining bone debris, wolf scats were collected throughout the period in which each carcass was consumed. During the initial 24-48 hour period, when primarily meat was being consumed, scats were very liquid and dark, and contained little or no bone. As bone and hide were eaten, scats became firmer, and hair and bone were often visible on their surfaces. Individual scats were cataloged and processed by water-screening through 1.6 mm (1/16 inch) mesh sieves to recover all bone fragments and hair. Some contained no observable bone or hair, others appeared to be composed primarily of tightly wrapped hair and little bone, and still others contained many identifiable and unidentifiable bone fragments. A total of 830 bone fragments larger than 6.4 mm (¼ inch) were recovered from a sample of 165 scats representing four deer carcasses; 260 of these specimens were identifiable to at least the element level.

The elements commonly occurring in scat (Table 2) reflect, to a large and perhaps predictable extent, those elements not present in recovered carcass debris. Carpals and the smaller tarsals made up approximately 6% of identified scat materials (Fig. 1, c,f).

TABLE 1. *Elements recovered from deer carcasses, 1986.*

Element	No. Expected	No. Observed	Percentage
Cranium	4	1	25.0
Mandible	8	8	100.0
Atlas	4	2	50.0
Axis	4	1	25.0
Cervical vertebra	20	3	15.0
Thoracic vertebra	52	2	3.8
Lumbar vertebra	24	7	29.2
Innominate	8	7	87.5
Rib	104	1	1.0
Scapula	8	4	50.0
Prox. humerus	8	2	25.0
Dist. humerus	8	7	87.5
Prox. radius/ulna	8	6	75.0
Dist. radius/ulna	8	0	0.0
Carpals	48	2	4.2
Prox. metacarpal	8	6	75.0
Dist. metacarpal	8	0	0.0
Prox. femur	8	2	25.0
Dist. femur	8	3	37.5
Prox. tibia	8	1	12.5
Dist. tibia	8	4	50.0
Tarsals	40	18	45.0
Fibular tarsal	8	4	50.0
Tibial tarsal	8	5	62.5
Prox. metatarsal	8	7	87.5
Dist. metatarsal	8	1	12.5
1st phalange	32	1	3.1
2nd phalange	32	0	0.0
3rd phalange	32	0	0.0

Number of elements expected based on four carcasses, observed equals elements recovered.

Twenty-five percent of identified elements were rib heads or shaft fragments, and 18.8% were vertebrae fragments. Distal metapodials, commonly missing from carcass debris, were represented in the scatological material by heavily pitted and eroded distal condyle fragments (Fig. 1, g). At least seven of 16 distal metapodials were identified in the scat materials. Phalanges (Fig. 1, h-i) and sesamoids made up 36.5% of identified bone from scats.

Alteration of bones due to consumption and erosion by digestive acids was also recorded. Three types of modification were observable macroscopically. Chunks of cancellous or compact tissue were often recovered, some of which still showed gnaw

TABLE 2. *Identifiable specimens recovered from wolf scats, 1986.*

Element	No. Identified	% Identified Material
Isolated teeth	2	0.8
Vertebrae fragment	49	18.8
Rib fragments	66	25.4
Costal cartilage	9	3.5
Forelimb	6	2.3
Carpals	10	3.8
Hind limb	5	1.9
Tarsals	6	2.3
Metapodials	12	4.6
1st phalange	6	2.3
2nd phalange	11	4.2
3rd phalange	11	4.2
Vestigial phalanges	32	12.3
Sesamoids	35	13.5

Percentages based on 260 identified specimens, representing four carcasses.

or puncture marks (Fig. 1, a-b). Additional puncture marks were probably masked by surface erosion of compact tissue. Many of these fragments have deer hair entangled in cancellous tissue, or firmly lodged in breaks and cavities (Fig. 1, b-c). A distinctive smoothing and rounded shallow pitting is common on compact bone surfaces. Some of these fragments show a high degree of polish or a sheen on their surfaces (Fig. 1, d-e). A number of specimens, particularly phalanges which are often swallowed whole, show eroding and thinning of compact bone tissue to the extent that the surface of the element is partially destroyed, and broken edges become nearly knife-edge thin (Fig. 1, h-i).

GRANITE QUARRY CAVE DEER REMAINS

Seventy-nine identifiable deer bones, representing at least four individuals, were recovered from test excavations within the cave (Table 3). Although no tooth-bearing elements suitable for ageing were recovered, 47 of the 79 elements (59.5%) were comparable in size to modern comparative skeletal materials from animals less than two weeks old and five to six months of age (Fig. 2). Wildlife studies have documented that wolves prey heavily on very young and very old ungulates, selecting the young particularly during the late spring and summer when they are most vulnerable. Thompson (1952:34-35) found remains of fawns in 45% of wolf scats, and remains of adult deer in 50% of scats collected during summer periods in Wisconsin. Sixty-one percent of scats collected in late summer contained remains of fawns, while only 31% contained adult deer remains, although he estimated the fawn to adult live population ratio to be approximately 1:3. Fawn hair occurred in 81% of wolf scats containing deer hair collected after May 31 in three years of study in Algonquin Park, Ontario (Voight *et al.* 1976:665), although the ratio of fawns to adults in the live population was assumed to be 1:1 or less. The high percentage (59.5%) of elements attributable to newborn or young of the year in the Granite

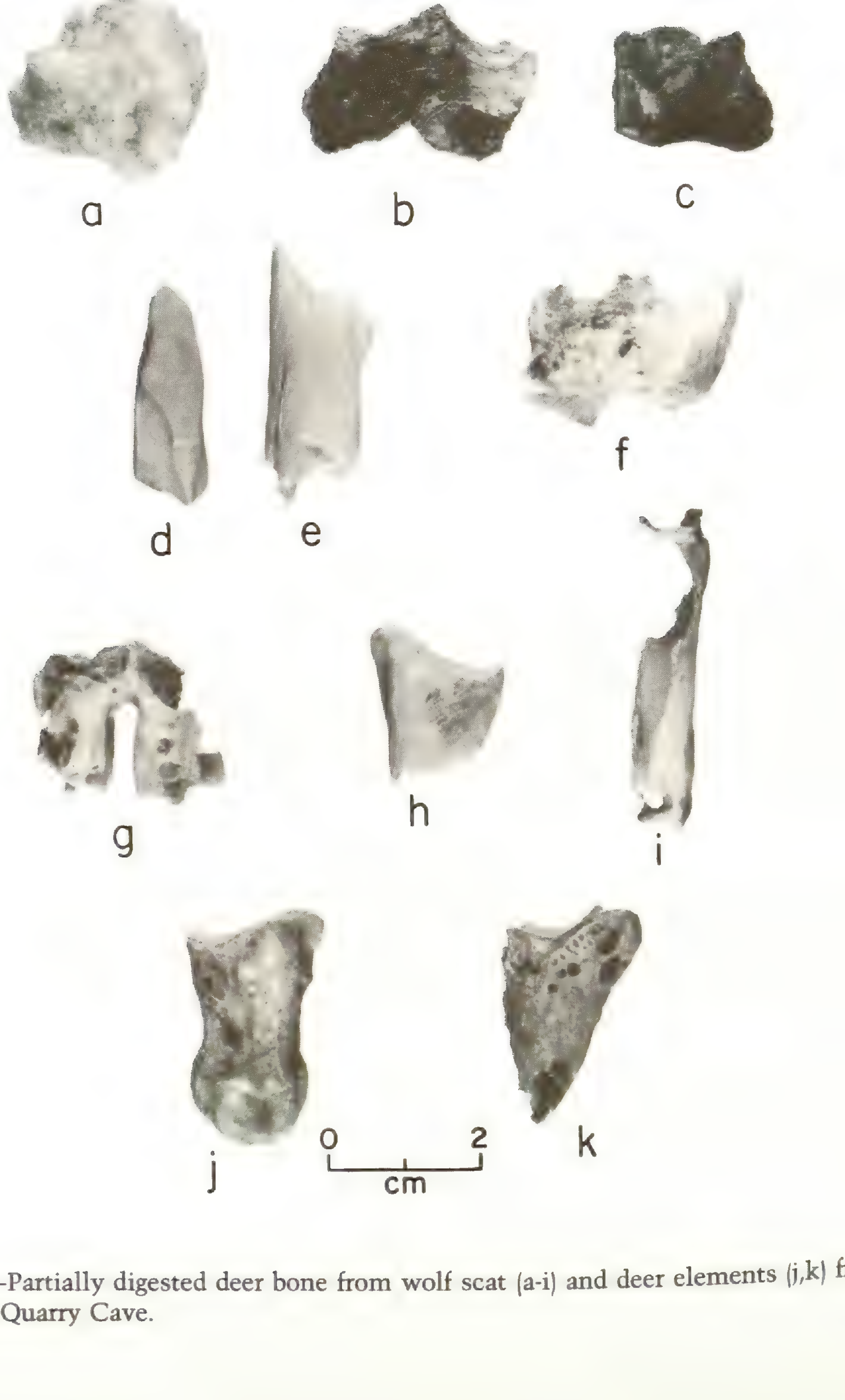

FIG. 1.—Partially digested deer bone from wolf scat (a-i) and deer elements (j,k) from Granite Quarry Cave.

TABLE 3. *Summary of identified mammal remains from the Granite Quarry Cave Site (23CT36), Carter County, Missouri.*

Taxa	Common Name	NISP	%NISP
Glaucomys volans	southern flying squirrel	1881	70
Insectivora	mole and shrew	269	10
Vespertilionidae	plainnose bat	160	6
Cricetidae	rat, mouse, vole, etc.	148	5
Odocoileus virginianus	white-tailed deer	79	3
Sciurus sp.	gray and/or fox squirrel	52	2
Geomys bursarius	plains pocket gopher	52	2
Procyon lotor	raccoon	13	<1
Sylvilagus floridanus	cottontail rabbit	13	<1
Marmota monax	woodchuck	7	<1
Tamias striatus	chipmunk	7	<1
Mephitis/Spilogale	striped & spotted skunk	7	<1
Didelphis marsupialis	opossum	4	<1
Canis sp.	wolf, coyote, dog	3	<1
Castor canadensis	beaver	1	<1
Vulpes/Urocyon	red or gray fox	1	<1
Felis concolor	puma	1	<1
Dasypus cf. *bellus*	giant armadillo	1	<1

Insectivora includes two species; Vespertilionidae, ≥five species; Cricetidae, ≥five species; other classes of vertebrates identified include: ≥seven species of fish; ≥fives species of amphibians; ≥13 species of reptiles; and ≥27 species of birds (Klippel, Snyder, and Parmalee 1986).

Quarry assemblage contrasts sharply with the sample from the nearby open Gooseneck Site (Smith 1975:28) in which no fawns were identified; 87.5% of the deer represented at the Gooseneck Site (based on MNI) fell within the 1-4 year age classes.

Based on element counts (NISP), the contrast in identified deer remains between the two sites is even more pronounced. Of 433 identified elements from Gooseneck, no fetal or neonatal specimens were noted and only 28 elements (6.5%) were judged to be those of juvenile or young adult animals (Mick 1982: Appendix C, 58-63).

Comparison of proportions of skeletal elements represented in the Granite Quarry assemblage with those recovered from wolf scats (Fig. 3) also suggests carnivores contributed to the archaeological assemblage. While frequencies of particular elements vary, it is notable that phalanges and sesamoids constitute 36.5% of the modern scat materials, 36.2% of young deer remains from Granite Quarry, and 32.1% of all Granite Quarry deer elements. Carpals and tarsals make up 6.0% of the Granite Quarry deer elements, and 6.1% of the modern scatological assemblage. Even more striking similarities between modern scat material and the Granite Quarry assemblage are evident in percentages of modification due to consumption and partial digestion by stomach acids. Clear evidence of pitting, thinning and erosion, and occasional gnaw marks occur on both adult deer elements (Fig. 1, j-k) and remains of very young animals (Fig. 2, b-c,f) from Granite Quarry Cave. Gnawing occurs on 6.0% of the Granite Quarry bone, 7.6%

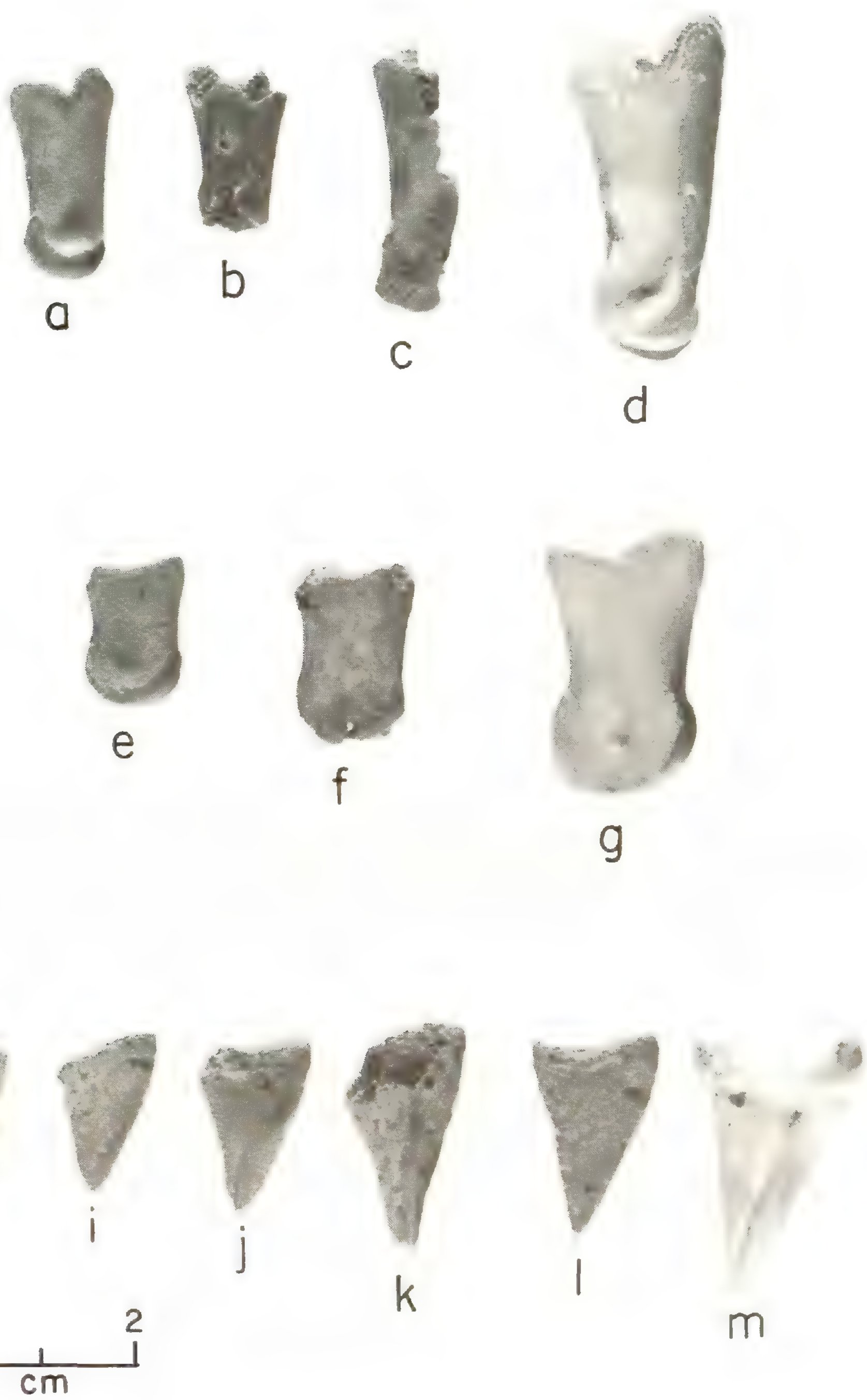

FIG. 2.—Deer phalanges from Granite Quarry Cave (b-c, f, i-l) and modern comparative specimens (UT Specimen No. 7238—0-2 weeks old: a,e,h; UT Specimen No. 7239—5-6 months old: d,g,m).

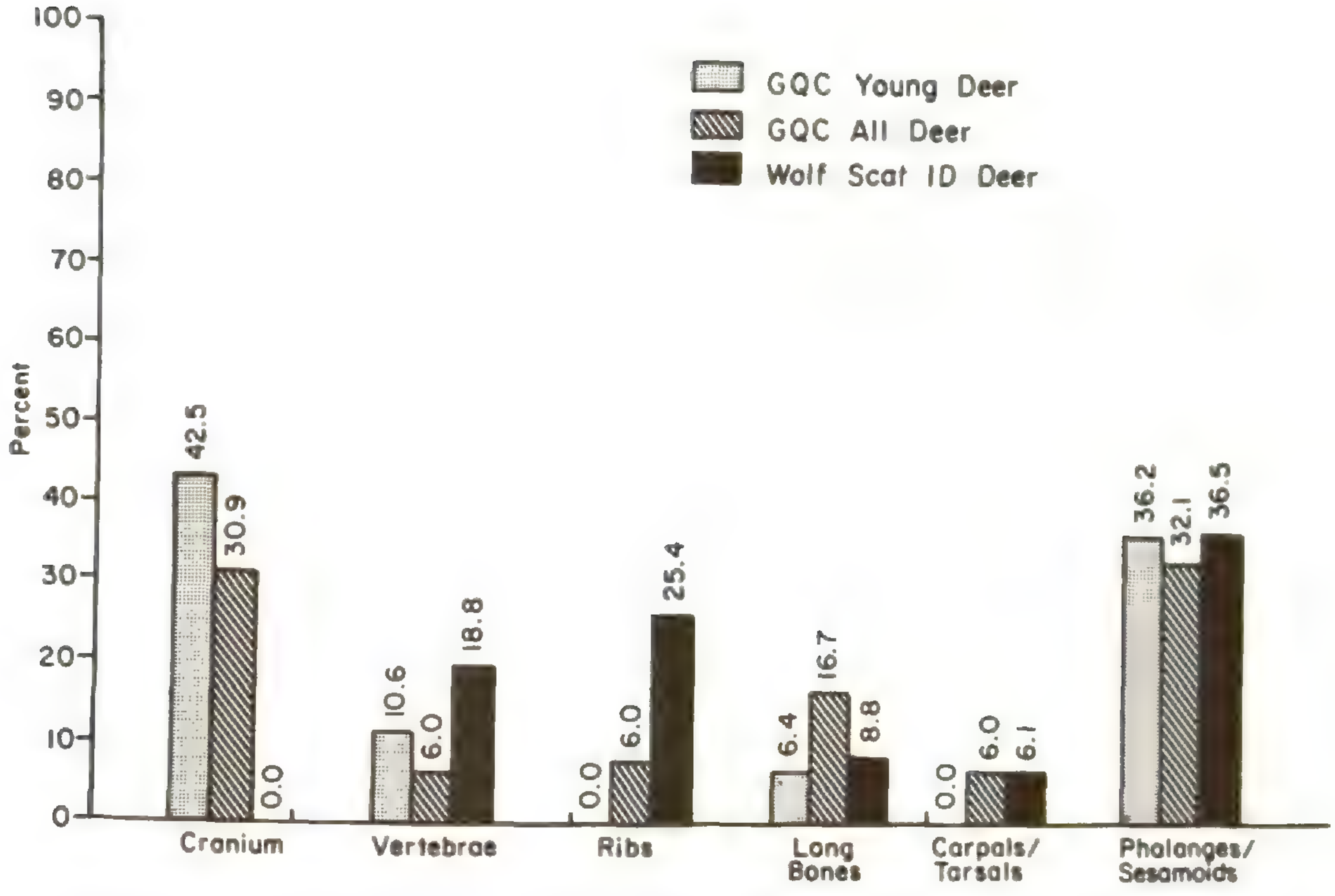

FIG. 3.—Identifiable deer remains from Granite Quarry Cave and modern deer elements from wolf scat.

of the modern scatological sample, and 4.2% of the identifiable bone from wolf scats (Fig. 4). Percentages of observed erosion and smoothing vary by less than 2% among the three defined groupings. Taken together, some form of observable alteration occurs on 32.1% of the Granite Quarry deer bone and 31.9% of the identifiable bone recovered from wolf scat.

The Granite Quarry deer bone assemblage is virtually identical to the scatological deer bone collected in the modern wolf study in degree of alteration due to consumption and erosion by digestive acids. This similarity, the high frequency of remains of very young animals, and the high relative percentages of phalanges and sesamoids in the Granite Quarry materials indicate that wild carnivores, perhaps wolves, probably were responsible for a substantial portion of the deer bone present in the cave deposit.

AVIAN PREDATION

Numerous bird species feed on other vertebrates; some consume and completely digest whole prey items while others regularly regurgitate undigested bone, hair, feathers, and scales. Raptors are known for their castings and among them owls tend to digest bone less than do hawks (Duke *et al.* 1975).

Some owl species regularly return to the same diurnal roost where subdued light is provided by coniferous trees, cavities in large trees, crevices in cliff or bluff faces, and in caves where irregularities in walls and ceilings provide potential roosts. The accumulation of reguritated owl pellets frequently produces a concentration of animal remains at such localities.

Control data.—In Middle Tennessee a wintering long-eared owl *(Asio otus)* returned to the same dead branch of a deciduous tree surrounded by the limbs of a red cedar *(Juniperus virginiana)* for three consecutive winters. During these periods the remains of an average

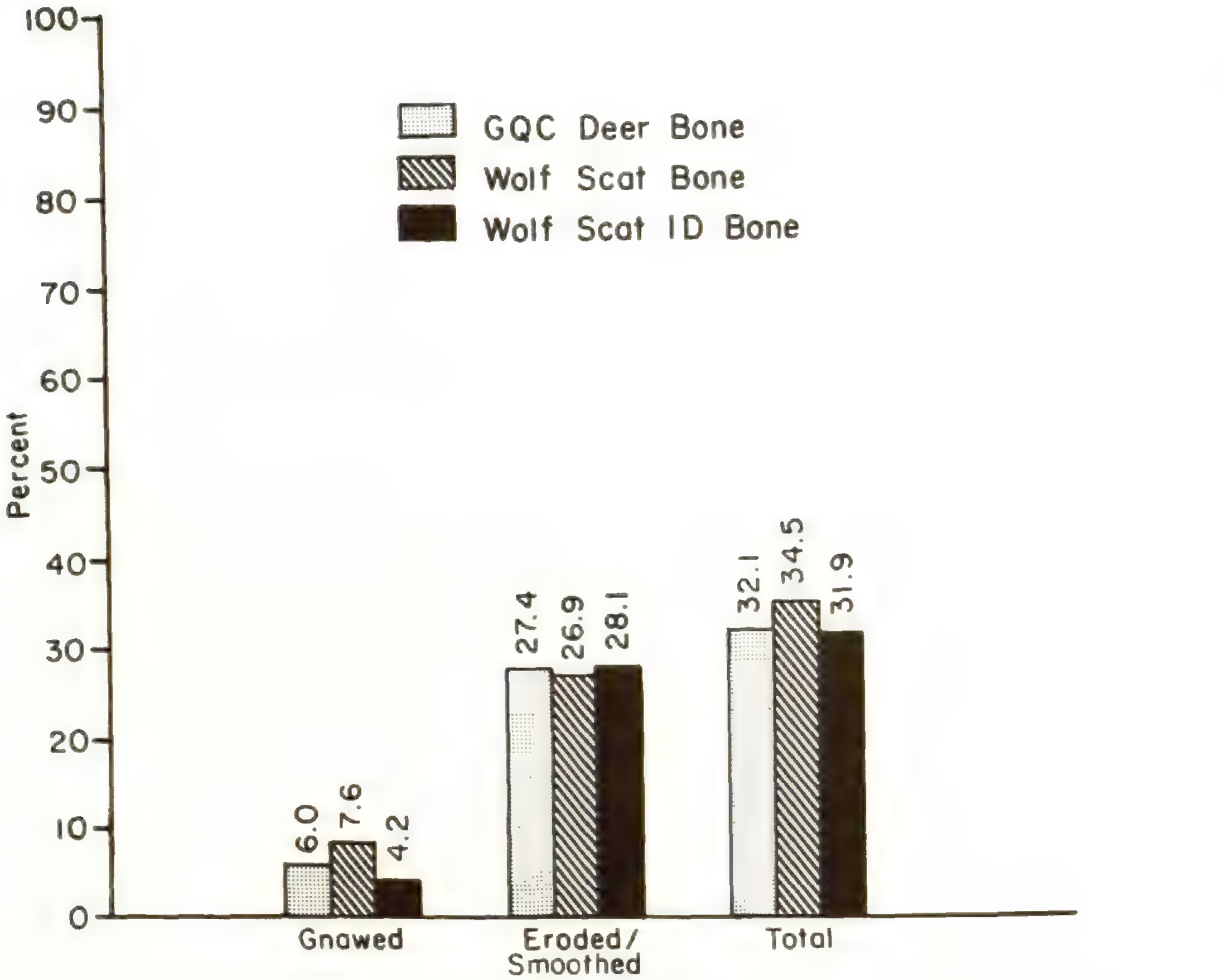

FIG. 4.—Observable alteration on Granite Quarry Cave deer remains and modern deer bone from wolf scat.

of 100 small mammals (MNI) accumulated each winter in an area of less than one square meter beneath the roost (Klippel and Parmalee 1982). Randle and Austing (1952:423) reported communal roosting of as many as 27 wintering long-eared owls in "a clump of four fifteen-foot pines" in southwestern Ohio. Hall and Blewett (1964:303) reported owl pellets and "a profusion of skeletal elements" accumulated by unidentified owls at the entrance of a cave in southern Missouri.

Studies of owl pellet contents have resulted in an extensive literature dealing with food preferences of various owl species inhabiting eastern North America. We have combined the results of our own owl pellet analyses with some of those in the literature for owls from eastern North America to develop estimates of prey size groups taken by six species of owls (*Bubo virginianus*, great horned owl; *Strix varia*, barred owl; *Tyto alba*, barn owl; *Otus asio*, screech owl; *Asio otus*, long-eared owl; and *Asio flammeus*, short-eared owl). All six inhabit large portions of the southeastern United States, at least seasonally, and all except *A. otus* and *A. flammeus* are year-round residents in the Southeast.

Because owls generally prey more heavily on mammals than they do on animals of other classes and because mammal remains are more frequently identified to the level of genus or species than are representatives of other classes of vertebrates, we have restricted our consideration of owl prey size to that of mammals. Sixteen sources were

consulted to obtain information on over 40,000 prey items for the six species of owls (Eaton 1934; Errington 1932; Errington *et al.* 1940; Huber 1960; Klippel and Parmalee 1982; Korschgen and Stuart 1972; Latham 1950; Mendall 1944; Pearson and Pearson 1947; Randle and Austing 1952; Richmond and Roslund 1949; Rusling 1951; Stupka 1931; Terres and Jameson 1943; and Wilson 1938). Sources that described fewer than 100 prey items were excluded.

Median adult weights for reported prey taxa were obtained from ranges provided by Schwartz and Schwartz (1981). It was necessary to defer to Burt and Grossenheider (1964) for median weights of three taxa *(Tamiasciurus hudsonicus, Glaucomys sabrinus,* and *Lepus americanus)* that do not occur in Missouri. Prey taxa were subsequently assigned to one of four arbitrary weight categories (0-100 g; 100-1,000 g; 1,000-2,000 g; and >2,000 g).

When relative frequencies were calculated by weight categories, over 90% of the mammal prey taken by five of the six owl species fell into the 0-100 g range (Fig. 5). The single predator species that deviated from this pattern was the great horned owl; the weight of over 60% of their prey exceeded 100 g. Ninety-nine percent of the mammals weighing between one and two kg in the great horned owl diet were lagomorphs (rabbits and hares). However, if lagomorphs are excluded from the great horned owl prey taxa, their predation pattern is similar to the other five widespread southeastern owl species in that nearly 90% of the prey they take, exclusive of lagomorphs, weigh less than 100 g (Fig. 5).

GRANITE QUARRY MAMMAL REMAINS

In addition to 79 white-tailed deer elements discussed above, 2,620 mammal elements representing 29 taxa were identified from the Granite Quarry Cave faunal assemblage (Table 3). Over 85% of the elements (NISP) identified were those of individuals weighing less than 100 g. When remains were grouped into the four size categories described above and relative percentages calculated, the resulting profile is strikingly similar to that generated for five of the six species of owls (except the great horned owl) that regularly inhabit southern Missouri (Fig. 5). Potential roosts for owls within the cave, in the form of "solution pockets and overhangs" on the walls (Price 1986), along with the small size of the mammals represented in the Granite Quarry sample, strongly suggests the preponderance of small mammalian faunal remains were probably accumulated by raptors.

The species composition of mammals within the 0-100 g range is further suggestive of noctural raptor predation in that over 75% of the elements in this weight category are those of the southern flying squirrel *(Glaucomys volans).* According to Schwartz and Schwartz (1981), *G. volans* is totally nocturnal; as a result they would have been especially susceptible to predation by owls. Errington *et al.* (1940:791) and Mendall (1944:205) have both noted relatively high incidences of flying squirrels taken by the barred owl. This owl tends to favor hunting in forested habitats (an essential component of flying squirrel habitat) and is one of the year-round residents of southern Missouri. Roughly 30% of the flying squirrel remains from Granite Quarry Cave were recovered from deposits that generally postdate prehistoric human occupation at the site, further indicating use by a nonhuman predator such as the barred owl.

Faunal assemblages have been identified from two other nearby sites along the Current River. Deposits from Limekiln Cave (23SH109) were screened through 6.4 mm mesh and artifacts representing Mississippian and Woodland components were recovered (Lynott and Monk 1985). A variety of screening techniques were employed at the open, Mississippian Period, Gooseneck site (23CT34), but Mick (1982) reported the larger than

6.4 mm fraction faunal remains separately from those recovered with smaller screen mesh. The same screen mesh of 6.4 mm used at both Limekiln and Gooseneck as well as at Granite Quarry has afforded us the opportunity to compare the faunal remains.

Identified elements from Limekiln, Gooseneck, and Granite Quarry have been grouped according to the same four size categories (Fig. 5). Although recovery techniques were the same for the three assemblages, the remains are strikingly different. Far greater frequencies of elements representing taxa over 2 kg were recovered from both Limekiln Cave (>55%) and the Gooseneck site (>90%) than from Granite Quarry Cave (<10%).

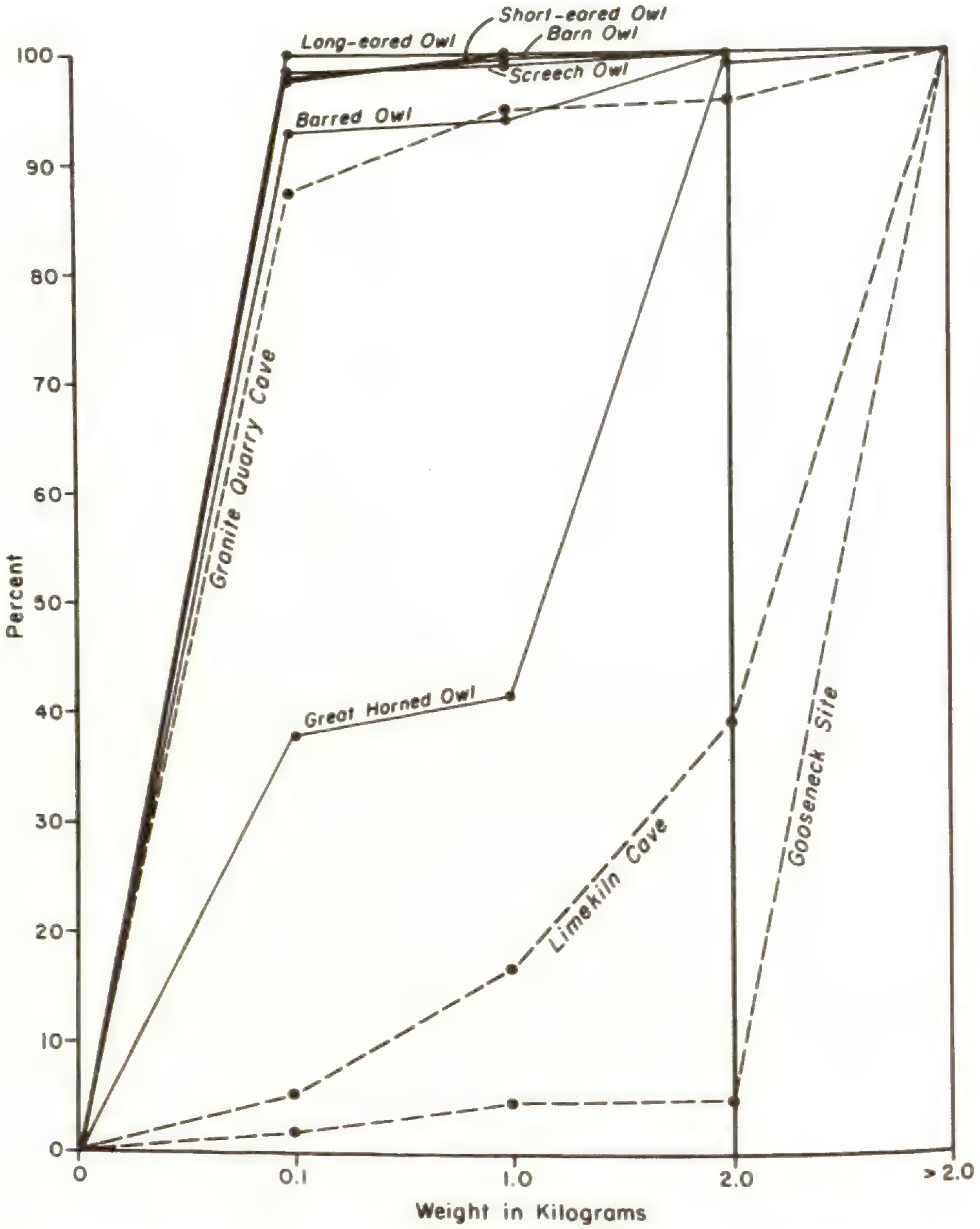

FIG. 5.—Size frequencies of mammals taken by great horned (n = 8,090), barred (n = 2,905), barn (n = 16,979), long-eared (n = 9,919), short-eared (n = 458), and screech owls (n = 1,997) compared to mammals (NISP) identified from Granite Quarry Cave (n = 2,699), Limekiln Cave (n = 105) and the Gooseneck Site (n = 416).

SUMMARY

Data collected in the modern wolf study indicate that certain skeletal elements are regularly consumed by wolves, and might be expected to accumulate in an area repeatedly used by these animals. Specific kinds of surface modification due to consumption and partial digestion are also documented. While this information does not provide a formula for assessing the degree of predator contributions to the Granite Quarry archaeological deer assemblage, the presence of these attributes, in proportions similar to those in the comparative sample from penned wolves, indicates a strong likelihood that a specific nonhuman agent has most likely affected this portion of the archaeological assemblage.

The majority of other mammal remains are also likely to have been introduced into Granite Quarry Cave deposits as a result of nonhuman activities. It has been well documented that some nocturnal raptors tend to haunt favored localities with subdued light, such as the twilight zones in caves, and that they are capable of accumulating large quantities of vertebrate remains in relatively short periods of time. It has also been demonstrated that these raptors generally prey on mammals of a fairly restricted size range (i.e. < 2 kg) and that the majority of the remains in some archeologically recovered faunal assemblages (e.g. Granite Quarry Cave) fall well within this range.

Notwithstanding arguments that point out the high biomass afforded by small mammals and their potential as food items in prehistoric human diets (e.g. Stahl 1982), it is suggested here that predators other than humans are potential contributors to faunal assemblages recovered from many archaeological contexts. The context, composition, and condition of the Granite Quarry Cave faunal assemblage clearly indicate substantial contributions by nonhuman agents.

ACKNOWLEDGEMENTS

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Isthmian Ethnobotanical Dictionary. Third Ed. Duke, J. A. Jodhpur, India: Scientific Publishers, Ratanada Rd., Box 91. 1986. Pp. xvi + 205, 325 illustrations. U.S. $60.00; Rs. 400.00.

With this inclusive dictionary, James Duke must enter the hall of ethnobotanical fame.

The need for this kind of book is manifest from the inability of the public to procure the first two editions. The author hopes that "many find it to be what they need to introduce them to the Isthmian Flora, a bewilderingly complex but exceedingly interesting composite of the North and South American floras."

This dictionary has as its basis many trips over many years by this botanist whose "childhood home" was Panama and to which he returns whenever his official duties in the United States Department of Agriculture allow him to do so. The data comprise those that have resulted from Duke's personal ethnobotanical research and those garnered from the literature.

A real dictionary, this production includes the Latin binomials, vernacular names, and uses of hundreds of tropical American plants. There are line drawings of many of the species enumerated. The species are arranged alphabetically by both common and scientific names, thus obviating the need for an index.

The wide public who should find this third and enlarged edition of value includes botanists and ethnobotanists, anthropologists, agronomists, herbalists, phytochemists, pharmacologically-oriented scientists, and others.

This contribution to ethnobotany is highly to be recommended.

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VIRTUOUS HERBS: PLANTS IN CHUMASH MEDICINE

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ABSTRACT.—The Chumash, like most other California Indians, believed that illness was largely a matter of power imbalance, or lack of harmony between the individual and the spiritual world. Most disease was thought to be caused by malevolent supernatural beings, by sorcery, or by violation of sensory or dietary taboos.

Plants played a role in most medical treatments. In traditional Chumash society, several kinds of shamans specialized in particular kinds of treatment for illness—smoke doctors, sucking doctors, ant doctors, herb doctors and others. All these highly trained practitioners were paid for their services and generally kept their methods secret. During the mission era, most of these specialties went into rapid decline and soon died out. The use of herbs then shifted, to become the basis of a widespread folk medicine whose application did not require formal status or training as a doctor.

This paper describes herbal medicine among the Chumash and discusses the effects of Spanish, Mexican and Anglo-American contact on these traditional practices.

INTRODUCTION

"We are constantly walking on herbs, the virtues of which no one knows."
—Chumash tradition (Hudson 1979:56)

At the time of initial European settlement in the late 18th century, the Chumash Indians of southern California were a group of about 15,000 people. Their ancestors had occupied the Santa Barbara Channel coast and offshore islands for 8,000 years or more. By the late prehistoric era they had become a classic example of "affluent foragers," with a complex, stratified society at the chiefdom level supported on a hunting-gathering-fishing economic base.

Among the elite members of Chumash society were a variety of occupational specialists who belonged to trade unions or guilds, the membership of which was based partly on wealth and partly on supernatural sanction. Makers of stone bowls, plank canoes, sinew-backed bows and certain other artifact classes each belonged to the appropriate guild and received pay for their products (Hudson, Timbrook and Rempe 1978:153-154).

Religious specialists also had their own organization, an esoteric ritual cult called the ***'antap.*** As in much of native America, the Chumash made little distinction between religious and political spheres, or between sacred and secular aspects of life. Members of the ***'antap*** group served as leaders of ceremonies, dancers, musicians, community chiefs and their assistants, and shamans (Hudson and Underhay 1978).

There were many kinds of shamans—bear shamans, weather shamans, astrologers, rattlesnake shamans and others—whose power was based partly on training but also on a close relationship with one or more supernatural spirit guides, or "dream helpers," acquired through visions (Applegate 1978). The same was true of shamans who were medical specialists, who used various pharmaceutical and therapeutic techniques to cure illness. There were smoke or pipe doctors, sucking doctors, ant doctors, herb doctors and others (Walker and Hudson n.d.).

These specialists were understandably quite secretive about the exact nature of the remedies they used. Their medical knowledge was the basis of their livelihood and may sometimes have been passed down in families from father to son (Geiger and Meighan 1976:73, 75).

Shaman doctors were probably not the only individuals who practiced medicine among the Chumash. Although there is little evidence to confirm it, a sort of folk medicine based on commonly known remedies, largely herbal, may have existed from the earliest times. The lack of information is not surprising, since explorers, missionaries and others who observed Chumash life in the early stages of European contact focused on what to them was strange and exotic behavior. The less spectacular, everyday practices were virtually ignored until John P. Harrington's work in the present century.

Most medical treatments, among both specialists and folk practitioners, made use of plants, the so-called "virtuous herbs." In this paper, I will describe a few of the most important plants the Chumash used in curing, and discuss changes that occurred in historic times. Sources consulted include explorers' journals, mission documents, late 19th century authors, John P. Harrington's extensive unpublished field notes and botanical collections, and more recent interviews.

PLANTS WITH LASTING IMPORTANCE

Yerba mansa (Anemopsis californica).—In the late 19th century, several authors in Santa Barbara and Ventura counties compiled information on medicinal herbs used by local Indian people and Spanish Californians. These early writers noted that a high value was placed on tea of yerba mansa root as a healing wash for cuts and ulcerated sores (Bard 1894:7; Bingham 1890:37; Birabent n.d.). Yerba mansa tea was also drunk for colds and as a blood purifier (Birabent n.d.; Benefield 1951:21).

Ethnographer-linguist John P. Harrington conducted interviews with Chumash people during the first half of this century, most actively from 1912 through the 1920s. Harrington's Chumash consultants called yerba mansa a "good medicine" (Maria Solares, Simplicio Pico, in Harrington n.d.). The root tea was drunk for cough (Luisa Ygnacio, Simplicio Pico), applied to cuts and sores (Luisa Ygnacio), or used as a hot bath for rheumatism (Maria Solares, in Harrington n.d.). Yerba mansa root was also chewed, drunk as tea, or inhaled to purify and strengthen a person who was to carry dangerous substances (Fernando Librado c. 1914, in Harrington n.d.).

Yerba mansa is quite often mentioned by early writers and Harrington's consultants as an effective remedy for venereal disease (Bard 1894:7; Fernando Librado c. 1914, in Harrington n.d.). After Spanish contact and colonization both syphilis and gonorrhea affected the Chumash, and these diseases devastated the mission populations. Yerba mansa, long used as a wash for sores, was logically applied to similar conditions. It was also taken internally in cases of venereal disease, probably being intended to aid by purifying the blood.

The Chumash believed that many diseases were really just one, blood trouble (Fernando Librado c. 1914, in Harrington n.d.). "Freshening" or purifying the blood, thereby restoring balance and harmony in the patient's body, was the goal of a number of treatments. Many of these were herbal, such as yerba mansa; other treatments, such as seawater drinking, were considered effective as well.

Today, Chumash people still consider the yerba mansa plant to be good for "cualquier dolor." They take yerba mansa tea as a blood purifier, use it to soak or wash infected cuts, and drink it for relief from colds, asthma and kidney problems (Gardner 1965:197; Weyrauch 1982:22).

Because of yerba mansa's reputed medicinal value, the plant has been intentionally spread by humans, and large patches have become established in new localities such

as the California islands (Smith 1976:107). Yerba mansa is reportedly used all over Hispanic America as a liniment for skin problems and as a tea for blood disorders (Jepson 1914:376; Ford 1975:341-343). Its popularity seems not to be a post-contact phenomenon, however, since yerba mansa is valued medicinally by Native American people in whose territory it occurs, and names for it exist in several Indian languages. I think its use among the Chumash dates from prehistoric times, although it may have expanded somewhat under the influence of Mexican-American folk medicine.

Chuchupate (Lomatium californicum).—Mission-era writers noted the importance of chuchupate to the Chumash, both for its medicinal value and as a sort of talisman. The Chumash habitually carried or wore a piece of the root, and soldiers with the Longinos expedition in 1792 purchased bits of it "at a good price" from the Indians (Longinos 1792, in Simpson 1961:46). What benefit the chuchupate was expected to confer to either Indians or soldiers was, unfortunately, not specified.

The Chumash practice of carrying chuchupate root on the person was observed in the following century as well. According to an author of the late 1800s, this was done in order to repel rattlesnakes (Bard 1894:9).

In the early 20th century, Harrington's consultants were familiar with the use of chuchupate to control rattlesnakes. The root was thought to be capable of stupefying the snake or attracting it if one wanted to capture it (Fernando Librado c. 1914, in Harrington n.d.); and a snake would rattle, but not bite, a person who carried a piece of chuchupate root in the clothing (Maria Solares 1918, in Harrington n.d.).

Medicinal uses of chuchupate were recorded in early mission times. The aromatic root was chewed for headache, chewed and rubbed on the body for any sort of pain, and the scent inhaled for headache (Longinos 1792, in Simpson 1961:45; Mission San Fernando 1814, in Geiger and Meighan 1976:73).

A late 19th century physician said that chuchupate root was chewed as a tonic, and "was useful in flatulence, headache, and neuralgia" (Bard 1894:6). For stomach trouble, it was taken as a decoction or soaked in a bottle of brandy and drunk (Birabent n.d.; Benefield 1951:24).

In the early 20th century, Harrington's consultants mentioned most of these same uses for chuchupate. For pain relief, the root was chewed and rubbed on; it was applied the same way or drunk as a tea for rheumatism (Fernando Librado, Juan Justo, Luisa Ygnacio, Maria Solares, in Harrington n.d.). It was also applied as a poultice to sores and used in a number of other ways; it was said to be very strong (Maria Solares, in Harrington n.d.).

Among modern Chumash chuchupate is still important but restricted to medicinal applications. These medicinal uses seem to have expanded somewhat, possibly influenced by outsiders. The fresh or dried root is chewed or made into tea which is considered "good for everything from diarrhea to toothache," coughs, sore throat, nausea, upset stomach and constipation (Gardner 1965:298; Weyrauch 1978:25). One person is said to carry pieces of chuchupate root around in his pocket "for whenever he feels bad," but none of the people recently interviewed mentioned any connection with rattlesnakes.

In the Great Basin, Southwest and Mexico, *Ligusticum porteri* is known as chuchupate or oshá (Ford 1975:182, 253-254; Bye and Linares 1986:290). That species is not found in the Chumash region, but its uses are strikingly similar to those reported for *Lomatium californicum* among the Chumash. The evidence indicates that medical and magical uses of the plant *(L. californicum)* have a long tradition in this area and probably were not introduced by Mexican Indian or Hispanic people in historic times.

PLANTS FORMERLY IMPORTANT, BUT NO LONGER USED

Datura (Datura wrightii)—In the sense that Native American medicine is integrally involved with religious beliefs and practice, datura [usually called ***toloache]*** probably was the single most important medicinal plant of the Chumash. This was recognized by the early missionaries, one of whom stated in 1814: "With this they intoxicate themselves. They take it in order to become strong, in order not to fear anyone, to prevent snakes from biting them and that darts and arrows may not pierce their bodies, etc." (Geiger and Meighan 1976:48).

Datura is, of course, a hallucinogenic plant and was used by the Chumash for three principal purposes (Applegate 1975): first, establishing contact with a supernatural guardian to provide protection, special skill and a personal talisman; second, contacting the dead, finding lost objects, seeing the future, seeing the true nature of people; and third, curing the effects of injury, evil omens or breaches of taboo, and providing immunity from danger. For example, a person whose form was assumed by a coyote was in grave danger and should immediately take datura to prevent soul loss and death.

Datura's visionary applications did sometimes play a role in medicine but are too elaborate for discussion here. It was administered by a specialist when one was seeking visions, especially the first time, and there were always concomitant restrictions on diet and sexual behavior.

As a medicine, datura was prepared by a relative, usually the mother or grandmother of the patient. The leaves or roots were taken internally as a tea and/or applied as a poultice for broken bones and wounds. This was used as the treatment of last resort when other methods failed, and was also thought to cure the ill effects of a narrow escape from death. Datura and seawater were considered the best tonics for freshening the blood (Applegate 1975).

Harrington's consultants provided some additional data. The four most important Chumash remedies were said to be datura, seawater drinking, cauterizing, and big red ants (Fernando Librado c. 1914, Maria Solares 1918, in Harrington n.d.). Datura was considered to be an effective remedy for tapeworm (Fernando Librado c. 1914, in Harrington n.d.). For broken bones, datura was taken internally (Fernando Librado, Simplicio Pico, Maria Solares, in Harrington n.d.). It is unclear whether this was intended as an anesthetic for setting bones or as a way of restoring spiritual balance after injury.

It was believed that datura played a role in death from snakebite. According to Maria Solares, when a rattlesnake plans to bite a certain person, it sinks its fangs into a ***toloache*** root and sucks up the poison; the person it bites will die quickly (Applegate 1975:11).

Today, the traditional use of datura to induce visions is barely remembered, and medicinal uses seem to be completely forgotten. "The present-day Chumash know that in the past people used ***toloache*** . . . It is said that it made one 'crazy'" (Gardner 1965:294). Older people in the late 1950s were aware that their ancestors practiced sweating and vaguely associate datura drinking with the sweathouse. This is thought to have made the men feel well (Gardner 1965:297). Actually, it was not datura but tobacco that was used in this way.

Datura was avoided by nearly all of the Santa Barbara area Indian and Hispanic people interviewed in the late 1970s. One unidentified informant spoke of inhaling the smoke from burning dried datura leaves as a treatment for asthma, although he had never used it for this himself. Most of the other people interviewed had no knowledge of datura use or said it was a dangerous drug; one said it was a form of liquor, a "bad medicine" (Weyrauch 1978:33).

These statements from older Chumash people indicate to me that traditional knowledge of datura use has not been passed down to the present generation from their elders. The use of datura as a hallucinogen may be undergoing a revival among some

younger Chumash descendants who are seeking to recapture their ethnic identity and follow ancestral traditions. It appears that current knowledge of datura use is based on personal experimentation and on information obtained from anthropological publications.

Tobacco (Nicotiana attenuata et al.).—Wild tobacco was formerly eaten not only as a recreational drug but also to promote good health. This practice made quite an impression on the Spanish explorers and missionaries; it is mentioned in several accounts and frequently likened to the alcoholic drinks with which Europeans were more familiar (e.g. Longinos 1792, in Simpson 1961:55). Priests at missions in Chumash territory stated that the Chumash used a mixture of wild tobacco and lime as an intoxicant, either chewed or drunk in water (Geiger and Meighan 1976:89).

According to Harrington's consultants, the wild tobacco was dried, ground and used plain or mixed with lime ash from burned shells. This mixture was called ***pespibata.*** Eating tobacco in the evenings after dinner, especially by gatherings of men, was a popular social recreation, "like people serve coffee or chocolate now." The resulting euphoria was generally accompanied by vomiting, which was considered a healthful practice (Fernando Librado, Simplicio Pico, Candelaria Valenzuela, Luisa Ygnacio, Maria Solares, Rosario Cooper, various dates in Harrington n.d.).

One consultant told Harrington that when people were still living at the missions, "the priests objected to Indians eating ***pespibata.*** At Santa Ynez the alcaldes [Mission Indian constables] used to go around looking for ***pespibata*** indulgers and would bring them in and whip them" (Hudson 1979:147-148).

Tobacco smoking was also practiced by the Chumash, but not in a recreational way. For example, in 1777 the explorer Fages witnessed tobacco smoke being blown over a corpse before burial (Priestley 1937:33-34). This is consistent with later information provided by Harrington's consultants. Only rarely, they said, were cane tubes filled with pure tobacco and smoked for pleasure. Smoking of tobacco in stone pipes was done only by specialists and intended as a blessing, for purification or for curing (Fernando Librado c. 1914, Maria Solares 1918 in Harrington n.d.).

The pipe doctor, an old man, was accompanied by two other old men, one a sort of messenger whose duty was to ascertain if anyone was sick and the other a singer. The pipe doctor blew tobacco smoke as an incense to treat the sick person. "A sick man would sometimes actually recover as a result of that treatment." There were special ways of handling the pipe, and the owner took an oath never to carry it around for fun or use it in play. He was paid for his services in chia, acorns, bead money, or whatever they had (Fernando Librado c. 1914, in Harrington n.d.).

Tobacco had a number of other medicinal uses. In 1814, a mission priest noted that one of the best known curative methods was a fermented mixture of wild tobacco, lime and urine, drunk to relieve stomach pain and to heal wounds (Geiger and Meighan 1976:73).[1] The same remedy was mentioned by Harrington's consultants: tobacco mixed with water was drunk to relieve pains in the stomach (Fernando Librado c. 1914, in Harrington n.d.) Walker and Hudson (n.d.) speculate that Chumash medicine may have emphasized purgatives and emetics for relief from internal parasites acquired from eating undercooked fish and sea mammals.

Tobacco was used externally as well as internally. The dried leaves were chewed or moistened with water and then rubbed on the body for topical pain relief, as for muscle soreness or when one's ears were being pierced (Fernando Librado c. 1914, Maria Solares 1918, in Harrington n.d.).

Neither medicinal nor ceremonial use of tobacco is mentioned in recent sources based on actual interviews (Gardner 1965, Weyrauch 1982). The medicinal uses of this plant which were formerly so important have been completely eclipsed by its use as a recreational drug. Many Chumash descendants do smoke tobacco nowadays. Some of

them smoke wild tobacco, often mixed with other substances, and for these individuals it may be symbolic of Indian identity.

Poison Oak (Toxicodendron diversilobum).—Poison oak was formerly an important Chumash medicinal plant. Mission records from the early 19th century state that plasters of powdered poison oak were very effective in healing wounds and lacerations (Geiger and Meighan 1976:75).[2] A hundred years later, poison oak juice was still being applied to staunch the flow of blood from a cut (Luisa Ygnacio c. 1914, in Harrington n.d.).

According to Harrington's consultants in the early 20th century, the juice from stems and leaves freshly cut in early spring was considered to be the most effective remedy for warts, skin cancers and other persistent sores. It was dripped onto the area and after contact with the skin turned black on exposure to air. When the black surface healed and fell off, the cancer was cured. This method was also used for calluses and corns on the feet (Fernando Librado c. 1914, in Harrington n.d.).

Poison oak was also taken internally as a remedy for dysentery or diarrhea. The root was boiled—taking care not to let the vapor get into the eyes lest blindness result—and the decoction drunk cold (Fernando Librado c. 1914, in Harrington n.d.).

Apparently Indian peoples generally did not experience the severe contact dermatitis which affects many contemporary Anglo Americans following contact with poison oak leaves and stems. In northern California, for example, the Pomo used the stems in basketry, the juice or ashes from burned leaves in tattooing, the leaves to wrap acorn bread for cooking (Chesnut 1902:364; Goodrich *et al.* 1980:82).

Harrington's Chumash consultants commented on differences in poison oak susceptibility even among Indians; the Yokuts, a neighboring inland group, were said to be severely affected by it when they visited the coast, but the local Chumash were affected only a little or not at all (Juan Justo, Fernando Cardenas, in Harrington n.d.). One woman said her mother used to work out in the field cutting poison oak and would "get it just a little on her arms" (Mary Yee c. 1955, in Harrington n.d.).

Immunity seems to have diminished along with the proportion of Chumash ancestry in modern descendants. Remedies for poison oak rash have been known for some time, but have become much more important in recent decades. Harrington's consultants treated the rash by rubbing on dry ashes from burned tule *[Scirpus acutus]* or rush *[Juncus textilis]* (Maria Solares c. 1918, in Harrington n.d.). Other remedies mentioned were bathing the area in lime water (used by the mission priests), mugwort tea *[Artemisia douglasiana]*, or nightshade leaves *[Solanum douglasii]* mixed with salt; or spitting on and rubbing the affected part (Fernando Cardenas, in Harrington n.d.).

By the late 1950s, Chumash descendants no longer used poison oak medicinally but eagerly sought remedies for its effects. The rash is bathed in a tea made from mugwort leaves, coyote brush *[Baccharis pilularis]* or coffeeberry *[Rhamnus californica]* (Gardner 1965:297-298). Currently, some believe that immunity can be obtained by spitting on the plant or by drinking a tea of boiled poison oak root (Weyrauch 1982:14).

PLANTS WHOSE USES HAVE CHANGED HISTORICALLY

Mugwort (Artemisia douglasiana).—Chumash doctors cauterized wounds of their patients with small cones made from dried mugwort leaves, which were placed on the skin and ignited. The cones slowly burned down, becoming hot and painful (Walker and Hudson n.d.:78). This method, similar to the moxa of Asian medicine, was also used to treat skin cancers and rheumatism. Older people often had many scars from the treatment, especially on their legs. ***'Apin,*** as it was called, was considered by Harrington's consultants to be one of the most important Chumash remedies, along with seawater, datura, blood-letting and red ants (Fernando Librado c. 1914, Maria Solares 1918, in Harrington n.d.).

The Chumash also practiced a variation on this technique, described by an early physician in Ventura (Bard 1894:9). A stone tube or pipe, heated by burning dried leaves, was held over the diseased spot to induce a blister. Stone tubes and pipes were restricted paraphernalia used by the sucking doctor and smoke doctor, respectively. If cupping or cauterizing with mugwort was originally employed by these specialists, it may be fairly ancient in Chumash medicine.

Harrington's consultants mentioned other medicinal uses of mugwort, including plasters applied for a sore neck and lying on a bed of the leaves over hot coals for paralysis [pasmo] (Luisa Ygnacio, Maria Solares, in Harrington n.d.). They also said it could be used as a remedy for headache and poison oak rash (various consultants, in Harrington n.d.).

Today, the best known use of mugwort is as a remedy for rashes, particularly as a wash for poison oak and measles (Gardner 1965:298; Weyrauch 1982:8-9). A tea of the leaves is drunk for headache and asthma (Gardner 1965:298), and also taken for severe fright (Juanita Centeno, pers. comm. 1978). The leaves are applied topically for headache (Gardner 1965:298) and toothache (Weyrauch 1982:9).

Among Chumash people interviewed in recent years, the cupping technique described by Bard (1894:9) was barely remembered by one Santa Ynez Chumash woman (Jennie Guillen): "They blew smoke or air into the cup and applied it to the person's back to take away pain, I believe." She saw this done in the presence of her grandmother, who had been one of Harrington's consultants in the early 1900s (Weyrauch 1982:2).

NATIVE PLANTS USED IN INTRODUCED DISEASES OR DISEASE THEORIES

In some cases, new uses were introduced or developed for plants which had already been part of Chumash medicine. This has already been mentioned in the case of foreign diseases or conditions which were brought to the Chumash after contact with Europeans. Yerba mansa, it was noted, became a specific for venereal diseases, and mugwort has become a preferred treatment for poison oak. Many traditional cold remedies were probably applied when influenza and pneumonia struck mission Indian populations.

New theories about the causes of illness were also introduced. Most important of these was the idea of hot-cold balance which is so prevalent in Hispanic medicine throughout Latin America and the Southwest. This belief seems not to have been indigenous to the Chumash, and although they adopted some of its methods they apparently did not internalize the theory behind it.

For example, one syphilis remedy described by a Harrington consultant entailed drinking a concentrated mixture of sea water and chile peppers; this "hot" remedy for a "hot" disease would never be used by a Latin American folk healer (Walker and Hudson n.d.:113). On the other hand, cutting a newborn infant's umbilical cord with a carrizo cane knife was consistent with this theory; in some areas metal or stone knives are considered "cold" and therefore injurious (Walker and Hudson n.d.:99). However, cane knives are sharp and the Chumash traditionally used them when skinning game animals and slicing deer meat (Maria Solares 1918, in Harrington n.d.).

PLANTS INTRODUCED ALONG WITH THEIR USES

In mission times and afterward, many new plants were brought into the Chumash area, both deliberately as garden cultivars and accidentally as weeds. Some medicinal plants were very obviously introduced, often along with recipes for using them, by people from Mexico.

Rue (Ruta chalapensis).—Rue was brought in as a garden and medicinal plant. In the late 19th century it was drunk as a tea for nerves and heart palpitations, and also used for ear problems (Birabent n.d.).

Harrington consultants did not mention this plant at all, but it has become well known among Chumash descendants today. Its most important use is for earache, the leaves being inserted into the ear either alone or with warmed oil (Gardner 1965:298; Weyrauch 1982:17). This is now one of the most common herbal remedies in the area.

Ruta is used medicinally for earache and deafness throughout the Hispanic Southwest and northern Mexico; sometimes it is also taken for abdominal pain and nerves (Ford 1975:293-294). These uses have been adopted by other California Indians (Bocek 1984:252; Hinton 1975:217).

Horehound (Marrubium vulgare).—Franciscan friars are said to have introduced this plant as a medicinal herb in the late 18th century (Jepson 1943:397-398). By the late 19th century it was abundant on roadsides and waste places, and much used for coughs and lung diseases (Bingham 1890:37). Harrington consultants said they sometimes made tea of horehound leaves, but did not consider it to be medicinal (Juan Justo c. 1925, in Harrington n.d.).

About 1960, horehound tea was vaguely remembered by some Chumash people as an old-time way of inducing abortion, and no other uses were recorded at that time (Gardner 1965:296-297). Currently it is most used as a cough medicine and for sore throats, colds and lung ailments, often mixed with honey (Weyrauch 1982:10).

Horehound has a wide variety of medicinal uses throughout Mexico and the Southwest (Ford 1975:237-238), but only the respiratory and throat remedies seem to have been adopted by Chumash descendants.

NEW PLANTS AS SUBSTITUTES FOR TRADITIONAL ONES

Introduced species which had their own distinctive uses are fairly easy to identify. It is more difficult to talk about new plants which may have replaced indigenous ones as remedies for particular ailments. Much depends on availability. The disruption of trade networks and restriction of access to private property have affected choices of medicinal materials. Mountain tea *[Ephedra viridis]*, valued for the blood and kidneys, grew in remote inland areas; horsetail *[Equisteum* spp.], which also has green, cylindrical, jointed stems, was much more common along the coast. Notes with plant specimens collected by Harrington's consultants indicate that horsetail may have been used medicinally when *Ephedra*, the preferred form, was not available.

Investigators in the 1920s and 30s suggested that one particularly desirable remedy, *Adenostoma sparsifolium*, called ribbonwood or yerba del pasmo, was formerly found in the Santa Ynez range and offshore islands, but overzealous collecting by early-day Mexican Californians exterminated it from these localities (Barber correspondence on file at Santa Barbara Museum of Natural History). In the 1920s, Harrington's consultant Lucrecia Garcia labeled a specimen of a different plant, *Haplopappus arborescens*, as yerba del pasmo. *Haplopappus* and other genera of Asteraceae are known as yerba del pasmo in Mexico and the Southwest, but locally I think this is not a matter of mistaken identity. It may be that when "real" yerba del pasmo *(Adenostoma sparsifolium)* became unavailable, people began gathering a similar-appearing plant which they had always considered "another kind" of yerba del pasmo *(Haplopappus arborescens)* and adjusted the folk category accordingly.

CONCLUSION

Medical practices are dynamic by nature, and culture change did not begin with European contact in the 1500s. Many remedies were probably learned from other Indian peoples in prehistoric times. The Chumash have always been quite willing to experiment with new remedies, both on the basis of information received from others and by trying whatever is at hand (Gardner 1965:297; Weyrauch 1982:1).

Today it is difficult to separate elements of traditional Chumash medicine from the folk medicine of the Mexican-American community since each group has learned from the other. The Harrington materials from the first part of this century include information on 95 Chumash medicinal plants, only 9 of which were introduced species (Timbrook 1984:144), and use of cultivated or store-bought remedies has increased since then. Nearly half of the 49 medicinal plants which a recent study listed as used by Indians of Santa Barbara County are non-native species (Weyrauch 1982).

Many Chumash have expressed a general distrust of the competence and motives of Western physicians. One of Harrington's consultants said, "The doctors nowadays are more for getting money than they are for curing people. They do not have the power of curing people like the old Indians had" (Harrington n.d.). This belief remains widespread today.

Since the traditional specialists—pipe, sucking, and other doctors—died out shortly after mission times without passing on their technical knowledge, folk medicine practiced by non-specialist individuals has played an increasingly important role. But the Chumash today still feel the need to consult expert practitioners of traditional medicine. In the absence of curing specialists on the Santa Ynez Reservation in the late 1950s, some individuals occasionally went to Mexican healers and Chinese herbalists (Gardner 1965:296).

"My grandfather told me that it was good to learn about medical herbs, but that people will die just the same. It is merely a pastime."

—(Fernando Librado, in Hudson 1979:56)

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NOTES

[1]These authors suggest "urine" may be a mistranslation of Spanish, and perhaps "water" is meant (Geiger and Meighan 1976:158). But urine was commonly used in folk medicine, according to Margarita Kay (personal communication 1987).

[2]Original document (Feb. 20, 1814): Los remedios que toman son de Plantas, Cortezas, Raizes, y ojas de Arvoles de los que no conozco ninguno sino es la Yedra con la que e visto hacer primores pues a un hombre hecho pedazos por un oso en brazos, piernas, costillas e spalda, con solo espolvonear la yerda [sic] sanó.

Although Geiger and Meighan (1976:75) translated yedra as "ivy," to Harrington's consultanta yedra meant poison oak. Bright also suggests this interpretation for the Luiseño term ***hial,*** called yedra in Spanish, translated by these autors as "ivy." The Luiseño drank the powdered root mixed with water as a purgative (Geiger and Meighan 1976:157).

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THE FOLK SUBGENUS: A NEW ETHNOBIOLOGICAL RANK

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ABSTRACT.—A seventh ethnobiological rank, the folk subgenus, is recognized and added to Brent Berlin's framework of principles of folk biological classification and nomenclature. A class affiliated with the subgeneric rank is monomially labeled and immediately included in a generic category. Identification of the folk subgenus removes certain terminological difficulties which arise when describing the growth of ethnobiological nomenclature. Most importantly, its recognition permits an elegant formal representation of folk biological taxonomy as empirically attested in several detailed field studies.

INTRODUCTION

In a recent article I (Brown 1986) presented several proposals concerning the growth of ethnobiological nomenclature. One of these proposals, that folk biological categories of the specific rank evolutionarily precede categories of the generic rank, was strongly challenged by Brent Berlin in his accompanying response to my paper. Berlin (1972), in an earlier pioneering work, proposed the privileged status of generic classes in folk taxonomies and their precedence in biotaxonomic development. One reason for my departure from this widely accepted view is that it presents certain terminological difficulties in describing details of the growth and development of folk biological taxonomy. However, I have come to recognize that my own proposal is also problematic with respect to terminology. The present essay attempts to resolve these problems and to reconcile my position with that of Berlin. It does so by recognizing a new ethnobiological rank, the folk subgenus.

BACKGROUND

In Berlin's (1972, with Breedlove and Raven 1973, 1974) framework, each biological class within a folk taxonomy belongs to one of six ethnobiological ranks. Berlin identifies fixed relationships between ethnobiological ranks and levels of taxonomic inclusion (Fig. 1).

The most inclusive class of a folk taxonomy belongs to the "unique beginner" or "kingdom" rank. For example, the unique beginner class in American English folk botanical taxonomy is *plant*. The unique beginner is associated with the first level of taxonomic inclusion or Level O. Classes affiliated with the "life-form" rank occur only at Level 1 in folk taxonomies. Examples of life-form classes from American English include *tree*, *vine*, and *bush*. Categories of the "generic" rank can also occur at Level 1 (not illustrated in Figure 1), but the vast majority of these are found at Level 2 and are subordinate to life-form classes of Level 1. Examples of generic classes are American English *oak*, *maple*, and *walnut*, which encompass organisms that are also kinds of *tree* (a life-form class).

Categories of the "specific" rank are immediately included in generic classes at Level 3 (or at Level 2 if pertinent generics are at Level 1). Examples are *white oak*, *pin*

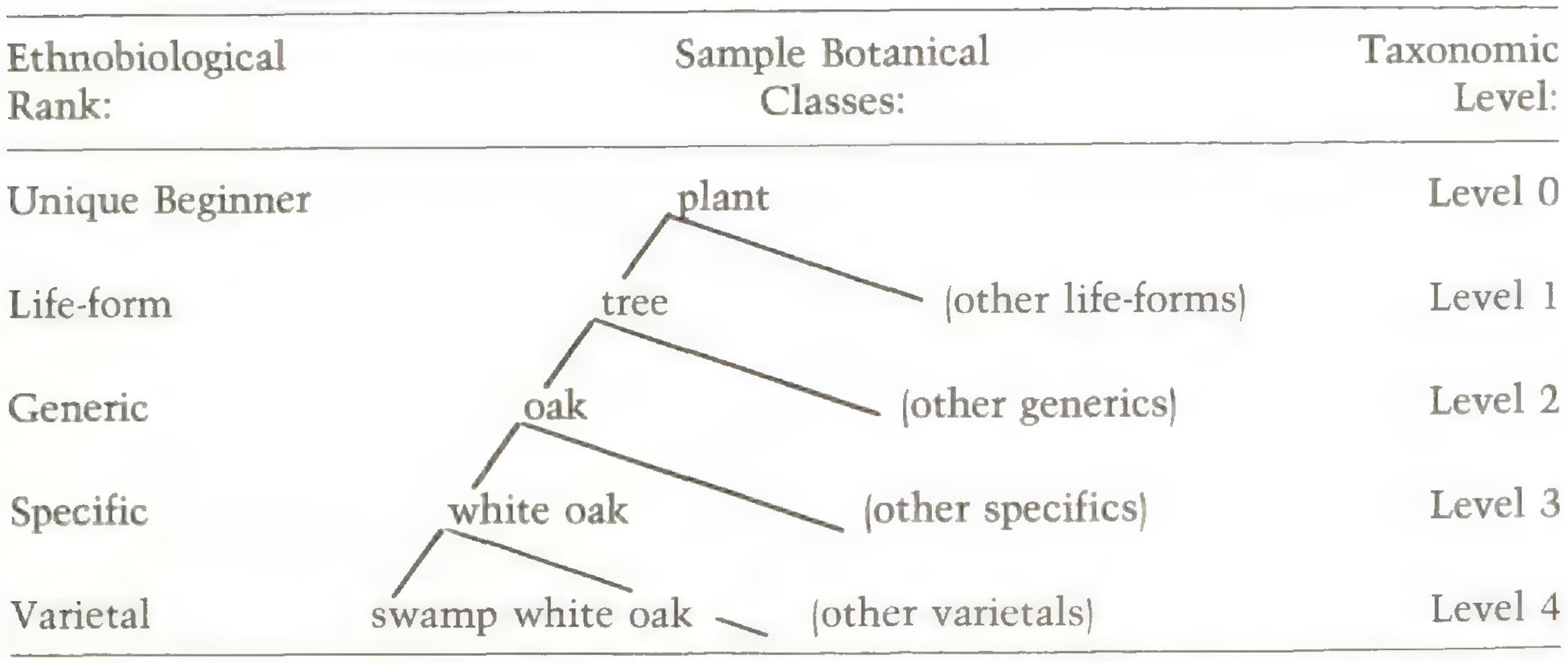


FIG. 1—Ethnobiological ranks and levels of taxonomic inclusion with sample botanical classes from American English.

oak, and *post oak*, all of which are kinds of *oak* (a generic class). Specific categories are occasionally partitioned into finer classes of a "varietal" rank, e.g., *swamp white oak*, a kind of *white oak*. The sixth ethnobiological rank, "intermediate," is not represented in Figure 1 since *labeled* intermediate classes are rare in biological taxonomies. Classes of the latter rank typically occur between life-forms of Level 1 and generics of Level 2.

Rank affiliation is determined by several other features in addition to taxonomic level. For example, classes of the unique beginner and life-form ranks are always *polytypic*, that is, they immediately include at least two labeled members. On the other hand, generic, specific, and varietal categories may be either polytypic or *terminal*. Terminal classes include no labeled members.

One important criterion of rank affiliation is linguistic or nomenclatural in nature. For example, life-form classes are labeled by *primary lexemes* (usually simple, monomial terms such as *tree)*. Generic classes are also labeled by primary lexemes. On the other hand, specific classes are always labeled by *secondary lexemes*. A secondary lexeme consists of the term for the class in which its referent is immediately included and a modifier, e.g., *white oak*, a kind of *oak*. If specific classes are further partitioned, they include varietal classes which are also labeled by secondary lexemes. A secondary lexeme in the terminology of contemporary linguistics is a "binomial label."

The above is only an outline of Berlin's framework, sufficient for understanding the discussion that follows. Readers wishing further detail are referred to Berlin, Breedlove, and Raven (1973, 1973).[1]

Berlin (1972) places the ethnobiological ranks described above in developmental perspective. He assembles evidence of several kinds attesting to a sequence in which languages typically encode ethnobiological ranks. He notes, for example, that classes of the generic rank are vastly more numerous in folk taxonomies than those of other ranks and that these categories are psychologically basic. Consequently, he proposes that the first biological classes to develop in languages are always generic. After generic classes are encoded, classes of either life-form rank or specific rank or both may emerge. Subsequently, taxonomies expand by encoding classes of either the intermediate or the varietal rank, or both. The unique-beginner class is the last to develop (Fig. 2).

RECENT INTERPRETATIONS

Recently, I (Brown 1986) presented a revised sequence for the addition of nomenclatural categories to languages (Fig. 3). The major departure of this sequence from

FIG. 2—Berlin's sequence for the addition of nomenclatural categories to languages (adapted from Berlin 1972).

that of Berlin is that I recognize specific categories as preceding generic ones. A principal reason for this revision relates to my recognition that polytypic generic categories typically develop through expansion of reference of terms for classes included in those categories. For example, the Navaho term ***kat*** labels a generic class consisting of three species of juniper (Berlin 1972:60). The same term is also used more restrictively to refer to one of these three species, i.e., "common juniper," while the other two species are labeled by secondary lexemes, i.e., ***kat-nee-ay-li*** "strained juniper" and ***kat-dil-tah'-li*** "cracked juniper." Use of a single term, such as ***kat,*** to refer to both a generic class and a specific member of that class is known as *generic/type-specific polysemy*. In my paper (Brown 1986) I compiled data strongly indicating that generic/type-specific polysemy typically develops through expansion of reference. For example, in the Navaho case, it is highly likely that ***kat*** referred initially only to "common juniper," and that it took on its generic application when it expanded to encompass all three species of juniper. Since polytypic generic classes typically develop through expansion of terms for specific classes (or type-specifics), it follows that classes of the specific rank typically precede classes of the generic rank, hence, the revision (Fig. 3).

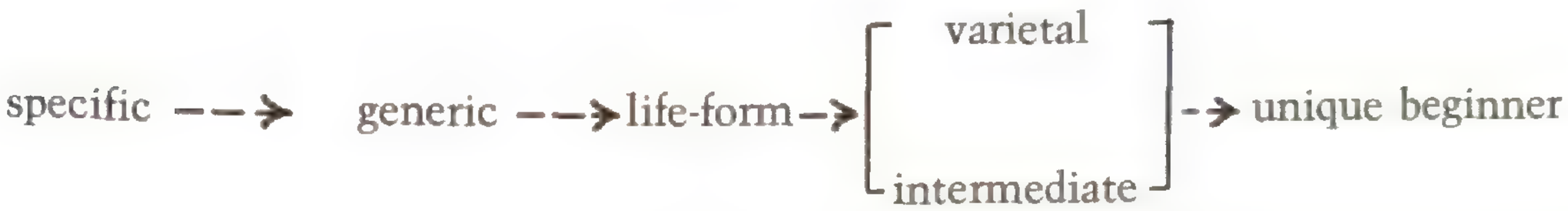


FIG. 3—Revised sequence for the addition of nomenclatural categories to languages (from Brown 1986).

Where Berlin and I differed significantly is in the interpretation of the rank affiliation of the class whose label expands to designate a more inclusive generic category of which it is a member. By the criteria of Berlin's framework, before expansion occurs, the category is affiliated with the generic rank. Thus, a term for a generic class referentially expands to a more comprehensive generic category. In addition, the original less inclusive generic class becomes a specific category (more precisely, a type-specific) when expansion occurs. In my framework, before referential expansion takes place, the labeled category is a folk specific and remains so after its label expands to the generic class of which it is a member. This avoids the terminological complication of having a *generic* category develop from another *generic* category with the latter changing in rank affiliation, i.e., from generic to specific. Under my interpretation, a generic category simply develops from a specific class and this involves no change in rank affiliation.

Another reason for my proposal relates to changes in folk biological classification and nomenclature that occur with a transition from a hunter-gatherer economy to an agrarian one (Brown 1986). Such a shift appears to involve the following developmental pattern: A relatively small number of plants and animals in a local environment are named by hunter-gatherers. These categories, which are designated by primary lexemes, for the most part are terminal and bear a one-to-one relationship to scientific species. In addi-

tion, they are readily perceptible to their namers without close attention (in other words, they are salient). With a transition to agriculture, this inventory of biological names and categories increases significantly in size (cf. Brown 1985). In addition, named, polytypic classes develop which encompass two or more, and sometimes many, related species. This is often achieved through expansion of reference of a biological label which originally was restricted to a single species. The emerging multispecies classes tend to acquire considerable salience, while the salience of labeled monospecies categories included in them tends to decrease.

In Berlin's framework, terminal classes of forager taxonomies are generics. This is so, despite the fact that they typically bear a one-to-one relationship to scientific species. I have proposed that these categories should be called specifics rather than generics in part because the word *specific* denotes a less comprehensive category than does *generic*. This proposal is also motivated by the observation that polytypic, multispecies classes which arise with a transition to farming typically develop through expansion of reference of terms for terminal, monospecies classes of foragers. In Berlin's framework the former classes are generic while, ironically, so are the polytypic, multispecies categories which develop from them. In my framework these are specific classes which, through expansion of reference of their labels, give rise to more comprehensive generic categories.

In his reply to my paper (Brown 1986), Berlin (1986) writes that I "treat all *terminal* taxa as *folk species*." In response to his comments I point out that I, in fact, only propose that terminal taxa occurring in biological taxonomies of hunter-gatherers be relabeled "specific" taxa, leaving open the possibility that terminal classes in agrarian taxonomies are generic. One reason for this is that I did not want to imply that *only* polytypic biological classes are to be recognized as affiliated with the generic rank. However, restricting my relabeling proposal to terminal classes of hunter-gatherers presents a serious problem. This is that in my framework a transition from foraging to farming entails that a specific taxon (*sensu* Brown), which is not dominated by a generic class, becomes a generic taxon (*sensu* Berlin) despite the fact that its range of reference does not change. Thus my proposal also leads to an unwarranted terminological complication.

THE FOLK SUBGENUS

From all of this I have concluded that neither Berlin's terminology nor the terminology I have proposed is sufficient for describing in an elegant manner the phenomena observed. As a consequence, I am abandoning my relabeling suggestion in favor of Berlin's original framework with one major modification. This modification is that a seventh ethnobiological rank be recognized, i.e., the folk subgenus. In brief, a subgeneric class is a taxon labeled by a primary lexeme which is immediately included in a generic class. This differs from a specific taxon, which is also immediately included in a generic class, in that a subgeneric is always labeled by a primary lexeme while a specific class is always labeled by a secondary lexeme.

The generalizations of Berlin's original framework (1972, Berlin *et al.* 1973, 1974) did not include the possibility that taxa labeled by primary lexemes could be immediately included in generic classes. The only classes recognized as being so included were those labeled by secondary lexemes and these, of course, are specific taxa. An exception to this aspect of his early formulation is recognition of the type-specific class as a specific taxon despite the fact that type-specifics are labeled by primary lexemes. In a latter publication (Berlin 1976), other exceptions were recognized. In fact, Berlin significantly altered his framework in 1976 when writing that "it now appears that where a generic taxon is further partitioned into specific classes, and one or more of the included species

are monomially designated (type-specifics excluded), *the monomial(s) will invariably refer to a taxon of major cultural importance''* (Berlin 1976:391-92).

Elsewhere I (Brown 1986:6) have cited several ethnobiological studies which document inclusion of monomially designated classes (i.e., taxa labeled by primary lexemes) in generic categories. However, contrary to Berlin's observation, these sources do not identify such taxa as typically being extraordinarily important or salient. Whatever their degree of cultural importance, it is now empirically certain that taxa labeled by primary lexemes are more than occasionally found to be immediately included in generic classes.

From the above quote of Berlin, it appears that he considers classes labeled by primary lexemes included in generics to be affiliated with the specific rank and thus essentially equivalent to classes labeled by secondary lexemes similarly included in generic categories. However, there are reasons for believing that these two kinds of classes are not equivalent, at least psychologically. Consequently, they should not be associated with the same ethnobiological rank.

As I have argued on two occasions (Brown 1985, 1986), folk classes of a taxonomy labeled by secondary lexemes (binomials) are overall less salient than categories of the same taxonomy labeled by primary lexemes. While I do not intend to repeat this lengthy argument here, I should mention my central hypothesis that binomial terms are more easily remembered or recalled than monomial terms (for lower-salience referents) and, consequently, are typically pressed into service as labels for lower-salience classes in order to enhance their "rememberability" (Brown 1985:51-52). On the other hand, since folk classes labeled by primary lexemes (monomials) are typically of higher salience than those labeled by secondary lexemes, they do not require rememberability enhancing labels.

A major difference between monomially designated classes included in generics and binomially designated classes included in generics, then, is that the former typically are considerably more salient that the latter. On the basis of this psychological distinction, I propose that these respective taxa are affiliated with two different ethnobiological ranks, respectively the folk subgenus and the folk species.

The folk subgenus is so named because in all but one respect it is identical to the folk genus. Like generic taxa, subgeneric classes are labeled by primary lexemes. If a generic class is immediately included in a labeled category, that category is always labeled by a primary lexeme. Similarly, subgeneric classes are always included in taxa designated by primary lexemes. Significantly, subgeneric classes demonstrate the high degree of pyschological salience characteristic of generic categories. The only difference between subgenerics and generics is that the latter are always immediately dominated by a generic class, while the former are never so dominated (but, as shown presently, a subgeneric class can be immediately included in another subgeneric class).

The subgeneric can be regarded as a kind of generic taxon. This logically necessitates recognition of an additional kind of generic class, i.e., those generic taxa which are never immediately included in another generic category or, in other words, the traditional generic of Berlin's framework. Thus the word "generic" takes on both a restricted sense and an expanded sense: respectively (1) as the traditional generic, and (2) as a category encompassing both the traditional generic and the subgeneric (Fig. 4).

[1]Traditional "generic" of Berlin's framework.

FIG. 4—Representation of the two types of generic classes.

The proposal of a subgeneric rank does introduce one complication into Berlin's framework which may actually help to resolve the long-standing problem of distinguishing life-form classes from polytypic generics occurring at Level 1 in folk taxonomies. Life-forms, in addition to always occurring at Level 1, are labeled by primary lexemes and immediately include taxa (generics) labeled by primary lexemes. Generic taxa occurring at Level 1 which immediately include subgeneric classes are, then, formally very similar to life-form classes, since they occur at Level 1, are labeled by primary lexemes, and immediately include taxa (subgenerics) designated by primary lexemes. Given this, the question arises of how one distinguishes life-form classes from Level 1 generics which dominate subgenerics?

There is, in fact, no clear-cut mechanical-like procedure for making such a distinction. However, there is a factor to take into consideration which points to one interpretation as opposed to the other. This is the occurrence in a contrast set of binomially labeled taxa along with monomially labeled taxa. For example, in Tzeltal (Chiapas highlands, Mexico) the word ***čan,*** which extends referentially to all true snakes, labels a Level 1 category that immediately includes 22 labeled classes (Hunn 1977:238). Of these 22, nine are designated by monomials, e.g., ***kantil*** "moccasin," and the remaining 13 are binomially labeled, e.g., ***haʔal čan*** "garter snake." In terms of the strict criteria of Berlin's original framework, ***čan*** is a life-form category. However, the fact that the majority of included taxa are labeled binomially suggests that ***čan*** is psychologically a generic taxon. This can be taken as evidence indicating that monomially labeled taxa included in ***čan*** are subgenerics rather than generics and, of course, that ***čan*** is a generic category rather than a life-form.[2]

I suggest, then, that, as a rule of thumb, a Level 1 class which immediately includes taxa labeled by both monomial and obligatory binomial terms should be identified as being a generic taxon rather than a life-form when the majority of included taxa are binomialy labeled.[3] This, of course, also means that included taxa labeled by monomials are subgenerics rather than generics. A corollary of this suggestion is that a Level 1 class should be regarded as a life-form category when the majority of included taxa are labeled by monomials. In such a case, taxa designated by monomials are generics rather than subgenerics.

I would also like to suggest another rule of thumb for distinguishing life-form classes from Level 1 generics that immediately include subgenerics. Life-form categories are typically the most polytypic classes of folk biological taxonomies (Brown 1984:14-19), which is to say that life-forms typically include immediately many more labeled categories than classes of other ranks (save the unique beginner). I would estimate that life-form categories recognized in terms of Berlin's strict criteria rarely, if ever, immediately include fewer than 10 labeled taxa. Thus, I suggest that Level 1 classes which immediately include fewer than 10 labeled taxa be regarded as affiliated with the generic rank, meaning that monomially labeled taxa included in such classes are to be regarded as subgenerics. A corollary of this suggestion is that categories immediately including more than 10 labeled taxa be regarded as affiliated with the life-form rank. Of course, given my above suggestion, this corollary will hold only when the majority of immediately included taxa are monomially labeled.

These proposals logically imply one further revision in Berlin's framework. In the latter he identifies two types of binomial labels: the productive primary lexeme and the secondary lexeme. A productive primary lexeme is a binomial which labels a class included in a contrast set which also contains taxa designated by monomials. On the other hand, a secondary lexeme is a binomial which labels a class included in a contrast set in which all other included classes (save a type-specific) are labeled by obligatory binomials. In terms of the 1976 version of Berlin's framework, generics can dominate contrast sets consisting of both monomially and binomially labeled classes. Also in terms

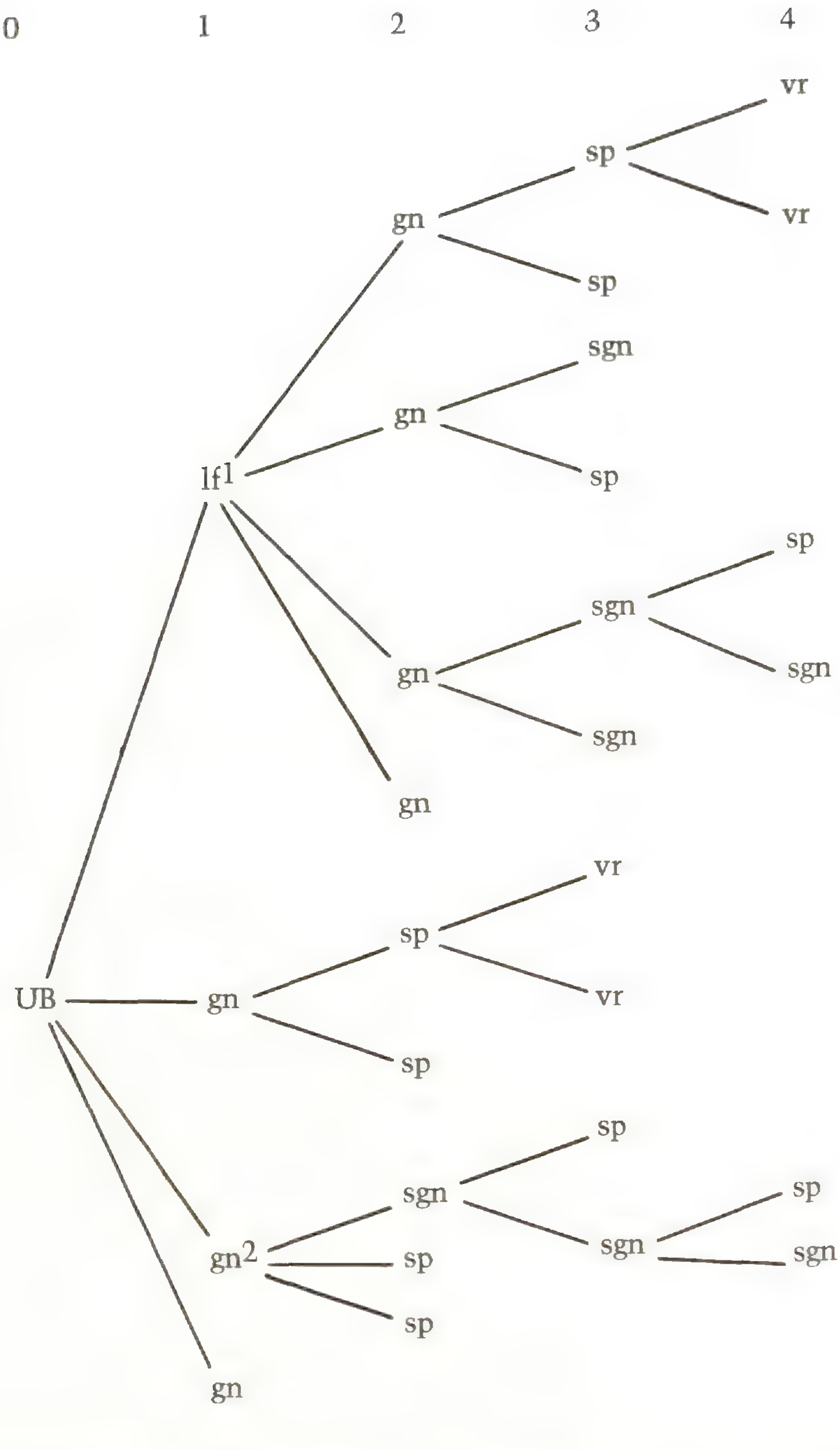
of this framework, when such a contrast set is in evidence, the binomial labels involved are productive primary lexemes rather than secondary lexemes. I suggest that such binomial labels be regarded as secondary lexemes and, further, that the taxa they label always be regarded as affiliated with the specific rank. Productive primary lexemes, then, will be restricted in designation to generic classes immediately included in life-form categories. *This revision entails that both subgeneric and specific taxa can occur in the same contrast set if it is dominated by a generic class.*

The above discussion is focused, for the most part, on Level 1 generics which immediately include subgenerics at Level 2. Subgenerics also occur at Level 3 in folk taxonomies and these are easily identifiable. They are those taxa labeled by primary lexemes that are immediately included in generics of Level 2 which are in turn immediately included in life-forms of Level 1. An example of Level 3 subgeneric categories comes from Tzeltal folk biological classification (Hunn 1977:xxix). The term ***č'o*** (roughly glossed "rat") labels a taxon which is immediately included in the life-form class ***čanbalam*** "mammal." The class it designates is, of course, generic. Twelve taxa are immediately included in ***č'o*** of which eight are labeled binomially, e.g., ***sabin č'o*** "rice rat," while only three are labeled monomially, e.g., ***sin*** "harvest mouse." The latter three taxa are, of course, subgenerics.[4] In addition, in terms of the revised framework the 12 taxa labeled by binomials are specifics.

An example of a Level 3 botanical subgeneric comes from Saami, a language of reindeer herders of Norwegian Lapland (Anderson 1978:519). The life-form class ***suoi'dni*** "bladed plant" immediately includes the generic category ***sinot*** which dominates two monomially labeled taxa which are, of course, subgenerics in terms of the revised framework. The first of these two taxa is ***giekka-njuolla*** which extends to the plant species *Deschampsia flexuosa* ("wavy-hair grass") and *Festuca ovina* ("sheep's fescue"). The second taxon, which encompasses any grazed species of families Gramineae and Juncaceae, bears the same label as the generic category in which it is immediately included, i.e., ***sinot.*** In terms of Berlin's original framework, the latter class is a type-specific. In terms of the revised framework, it is a subgeneric whose label happens to designate the class in which it is immediately included. To keep terminology more in line with the revised framework, I suggest that the so-called "type-specific" be renamed "type-subgeneric." Additional motivation for this proposal is discussed presently.

Saami provides another example of Level 3 subgeneric taxa which entails inclusive relationships mediated by a class of the intermediate rank (Anderson 1978:457). The life-form ***guolli*** "fish" immediately includes the intermediates ***sái'va-guolli*** "fresh water fish" and ***mearra-guolli*** "ocean fish." A number of generic taxa are immediately included in ***sái'va-guolli*** at Level 2 including ***čuov'ža*** which extends to any member of the genus *Coregonus* ("whitefish"). The latter term also labels a type-subgeneric *(C. lavaretus)* at Level 3 which occurs in a contrast set with at least two other subgeneric taxa, ***roabát*** *(C. vandesius)* and ***reas'ka*** *(C. albula).*

The subgeneric is similar to the traditional generic of Berlin's framework in that subgeneric classes too can immediately include classes labeled by binomials or, in other words, specific taxa. Ndumba (Papua New Guinea Highlands) provides an example (Hays 1974). The life-form ***mauna*** "small herbaceous plant" dominates over 60 generic taxa at Level 2. One of these, ***heng'gunru,*** encompasses various species of the genera *Alpinia, Curcumba, Hornstedtia,* and *Riedelia* which fall into 14 labeled subgeneric classes at Level 3 (Hays 1974:393). One of the latter, ***kaapi*** *(Curcumba longa),* immediately includes three labeled taxa at Level 4, one of which, ***fai'kaapi,*** is designated by a binomial (secondary lexeme) and is, consequently, a specific taxon. As it happens, the other two members of this contrast set, ***kaapi*** and ***kwaam'bimba,*** are monomially labeled. These are examples of subgeneric taxa which are immediately included in a subgeneric class,



UB = Unique beginner; lf = life-form; gn = generic; sgn = subgeneric; sp = specific; vr = varietal.

[1]As a rule of thumb this taxon is a life-form class only if it immediately includes more than 10 taxa, the majority of which are monomially labeled.

[2]As a rule of thumb this taxon is a generic class only if the majority of immediately included taxa are binomially labeled.

FIG. 5—Schematic representation of the relationships of the six universal ethnobiological ranks and their relative hierarchic levels as thus far empirically attested.

a relationship which should be formally incorporated into the revised framework. ***Kaapi*** at Level 4, of course, is a type-subgeneric.

In the Ndumba example, a Level 3 subgeneric dominates both specific and subgeneric classes at Level 4. Apparently subgenerics found at any taxonomic level can be partitioned into specific and subgeneric categories. For example, in Tobelorese (Halmahera Island, Indonesia) a subgeneric class of Level 2 immediately includes both specifics and subgenerics at Level 3 (Taylor 1980:391-392). In this case the Level 2 subgeneric, ***o kohutáa*** "banana type," is indirectly included in the Level 1 generic, ***o bole*** "banana," through an intermediate category, ***o bole (ma ca)*** "good banana." This subgeneric immediately includes ***o kohutáa,*** a type-subgeneric, and ***o kohutáa-galela,*** a specific. As it happens, the type-subgeneric is partitioned at Level 4 by two specific taxa, ***o kohutáa ma gare-garehe*** "white banana type" and ***o kohutáa ma doka-dokara*** "red banana type."

Tobelorese also offers a complex example in which three subgenerics are hierarchically related (Taylor 1980:452). A Level 1 generic, ***o tahubí*** (also known as ***o kaboja),*** encompasses all varieties of *Manihot esculenta* ("cassava"). It immediately includes at Level 2 the subgenerics ***o tahubí*** (a type-subgeneric) and ***o inggerehe.*** The Level 2 type-subgeneric is partitioned into ***o tahubí*** (a type-subgeneric) and ***o karet*** at Level 3. Finally, the Level 3 type-subgeneric immediately includes 14 subgenerics at Level 4, none of which, by the way, bear the label ***o tahubí.***

Figure 5 is a schematic representation of the relationships of the six universal ethnobiological ranks (including the new subgeneric rank, but not the intermediate) and their relative hierarchic levels as thus far empirically attested in folk biological taxonomies studied in detail. In presenting Figure 5, I am not proposing that these associations exhaust the range of relationships that actually occur. For example, it is not inconceivable that some specific classes immediately included in subgenerics may be found to be partitioned into varietal taxa in some languages. However, since such a relationship has yet to be observed, Figure 5 depicts all specific taxa as being terminal if immediately included in subgenerics.

PROBLEMS RESOLVED

Recognition of the folk subgenus resolves several problems in describing patterns in the growth of ethnobiological nomenclature. For example, in terms of the old framework, monospecies generic classes become specific categories (more precisely type-specifics) when words for them expand referentially to create multispecies generic classes despite the fact that their membership does not change. In other words, generic classes inelegantly become specific classes. In terms of the revised framework, when a word for a monospecies generic class expands referentially, creating a multispecies generic category, the monospecies generic simply becomes another kind of generic, i.e., a subgeneric (more precisely a type-subgeneric) or, in other words, a generic category immediately included in another generic category. Generic classes, then, elegantly remain generic classes after expansion of reference.

By abandoning my (Brown 1986) relabeling proposal in favor of a revised framework of which the subgeneric is a component, another terminological problem is solved. The proposal to which I refer is my suggestion that terminal classes occurring in taxonomies of hunter-gatherers be relabeled "specific." The problem with this proposal, as mentioned earlier, is that it implies that a specific taxon *(sensu* Brown 1986), which is not dominated by a generic class, becomes a generic taxon with a shift from foraging to farming despite the fact that its range of reference does not change. In terms of the revised framework, such a class is generic, not specific, and remains generic after a transition to agriculture.

An important aspect of the revised framework is that it buys us the ability to deal elegantly with systems in flux. The fact that it permits both subgeneric and specific taxa

to belong to the same contrast set is particularly significant in this respect. As I have outlined elsewhere (Brown 1986:6-9), when generic/type-subgeneric polysemy (formerly, generic/type-specific polysemy) develops through expansion of reference, the generic class may initially include several taxa labeled by primary lexemes (monomials) in addition to the type-subgeneric. As folk biological taxonomies grow in size and the salience of taxa in general decreases significantly (cf. Brown 1986), there is a tendency for such labels to be replaced by binomials, especially if the classes they designate decrease in salience. This creates the empirically attested situation in which a generic class immediately includes both taxa labeled binomially and taxa labeled monomially. In terms of the old framework, both types of taxon belong to the same rank, i.e. the specific rank, despite that binomially labeled classes usually are considerably less salient than monomially labeled classes. Formally recognizing these as being specifics and subgenerics respectively of the same contrast set captures this psychological difference, a difference which reflects a system in a state of change.

There is a tendency over time for all monomially labeled classes of a contrast set dominated by a generic taxon to develop binomial labels. Indeed, most polytypic generic classes reported in the literature immediately include (1) taxa, all of which are binomially labeled or (2) taxa, all of which are binomially labeled save the type-subgeneric. Indeed, it is this empirical observation that motivated Berlin to propose initially that all classes which partition a generic category are labeled by secondary lexemes (save the type-subgeneric).

It appears that typically the type-subgeneric is the last monomially labeled class of a contrast set dominated by a generic taxon to receive a binomial name (cf. Berlin 1972). This almost certainly relates to the fact that the type-subgeneric is typically the most salient category of a contrast set. Indeed, as is well understood now, a labeled taxon which becomes a type-subgeneric (after expansion of reference of its label to a more comprehensive generic class of which it is a member) is chosen for referential expansion because of its special salience (cf. Berlin 1972). It is, then, entirely appropriate to identify such a class as being a generic taxon (more precisely, a type-subgeneric) rather than, as in Berlins' original framework, a specific class (more precisely, a type-specific) which is typically low in salience relative to generic classes.

The revised framework also permits a more elegant treatment of changes involving Level 1 categories. It is probably the case that polytypic generics of Level 1 occasionally develop into life-form categories and vice-versa. Consequently, there will be points in times in which Level 1 categories will demonstrate properties of both life-form classes and polytypic generics. The Tzeltal Level 1 category cited above, ***čan*** "true snakes," is a case in point. Recall that of the 22 labeled classes immediately included in ***čan,*** nine are designated by monomials and 13 by binomials. Of course, in terms of Berlin's original framework, ***čan*** is a life-form class. On the other hand, I have suggested the rule of thumb that a level 1 category which immediately includes taxa labeled by both monomial and binomial terms should be identified as being a generic taxon rather than a life-form when the majority of included taxa are binomially labeled. By this rule of thumb, ***čan*** is a generic category. In addition, it is a generic class that immediately includes both specific classes (which are identified by their binomial labels) and subgenerics. By allowing Level 1 generics such as ***čan*** to encompass both specific and generic (more precisely, subgeneric) taxa at Level 2, the revised framework captures the possibility that such categories may be shifting rank affiliation from life-form to generic or vice versa.

My rule of thumb is, of course, an arbitrary convention. When a Level 1 category is in transition between life-form and generic ranks, it is in fact neither a full life-form class nor a full generic class but rather something inbetween. This is the case despite the answer yielded through reference to my rule of thumb. The revised framework deals

with this ambiguity in a clearcut manner while the original framework cannot handle it at all.

Other changes can occur in folk biological classification and nomenclature that can be handled by the revised framework but not by the original rendition. Consider, for example, Tobelorese classification and naming of rays (Taylor 1980:257-258). Two patterns are observed. In the first, the generic term ***o noara*** "ray" is immediately included in the life-form ***o nawoko*** "fish" and immediately includes two subgeneric taxa, ***o gugudai*** and ***o gorohutu,*** each of which designates a species of spotted fantail ray. The second pattern is the same as the first except that an additional class, labeled by ***o gugudai,*** mediates between the two subgenerics and ***o noara*** "ray" such that ***o gugudai*** and ***o gorohutu*** are identified as being types of ***o gugudai.*** What is probably occurring in this example is that some speakers of Tobelorese have developed a new subgeneric class which encompasses the two classes of spotted fantail ray by referentially expanding a label for one of these two classes, i.e., ***o gugudai,*** which, of course, becomes a type-subgeneric. Lacking a folk subgeneric rank, there is no way in which such a change can be similarly dealt with in Berlin's original framework.

CONCLUSION

The revised framework developed in this paper would seem to resolve the terminological problems outlined earlier involved in describing aspects of the growth of ethnobiological nomenclature. In addition, it would seem to reconcile my position with that of Berlin by allowing for the privileged status of generic classes (of two types, see figure 4) and their precedence in biotaxonomic development (figure 2 is valid, while figure 3 is not). However, most important is that recognition of the folk subgenus permits us to represent in an elegant, formal manner folk biological classification and nomenclature as empirically attested in detailed field studies.

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NOTES

[1]It should be noted that Berlin's framework has been subject to considerable criticism, especially during the past five years or so. Ellen's (1986) recent paper in this journal cites most of this critical literature and provides an additional negative assessment. The issues raised are too numerous and complex to be summarized here. However, as I hope to argue in detail in a future paper, this criticism is largely directed at possible (but not always probable) implications of Berlin's framework (which Berlin himself would not have necessarily drawn) and does not directly challenge the empirical findings on which his studies are based or the specific generalizations that Berlin has derived from them. In any case, there is one key component of the framework that has rarely been challenged or even discussed by critics, this being Berlin's recognition of nomenclatural principles in folk biology. (For example, these principles are not discussed critically or otherwise by Ellen [1986] or by Hunn [1982] in an earlier important review of Berlin's work.) In my view, Berlin's recognition of nomenclatural regularities is a major and, almost certainly, enduring contribution to ethnobiological science. Indeed, until these principles are proved invalid (which is highly unlikely), Berlin's framework, despite its possible implications and interpretations thereof, cannot be dismissed or ignored by scholars who have agenda in folk biological classification.

[2]Despite Berlin's strict criteria, Hunn (1977:238) identifies *can* as a "named complex" rather than as a life-form class. In a personal communication, Hunn has indicated to me that use of "life-form" for the category was rejected on the grounds that nomenclatural evidence indicates that the class is psychologically more like a generic taxon than a life-form category.

[3]Some binomial terms are optionally applied to categories while others are obligatory in application. For example, the binomial label *maple tree* is optional since its truncated form, *maple*, is equally appropriate referentially. On the other hand, the obligatory binomial *white oak* cannot be similarly truncated to *white* and still maintain the same botanical referent.

[4]One might want to argue that *c'o* "rat" is a rare example of a *labeled* intermediate category. That the majority of taxa immediately included in *c'o* are binomially designated strongly suggests that the class is psychologically generic rather than intermediate.

The Rice Economies: Technology and Development in Asian Societies. Bray, Francesca. New York: Basil Blackwell. 1986. Pp. xvii + 254. $24.95 (hardcover).

Francesca Bray believes that the social and economic history and technological development of Asian societies have been misunderstood by Eurocentric economists and analysts. The result of this ignorance has been the consideration of these societies as "backward," "primitive," and incapable of or unresponsive to improved growth and development through modern technologies. The cause of these misconceptions is cultural-(specifically, economic- and technical-) chauvinism; a model of technological development derived from Euro-American agriculture and capitalistic industrial systems has become *the* model for all such development everywhere.

People and societies who live by growing wet rice are different from the European model, says Bray. She proposes an alternative approach to technology and development, based more on intensification of labor and the specific skills of the producer than on capital investment and mechanical hardware: "I hope to show that societies which depend for their subsistence on wet-rice cultivation have in common a basic dynamic of technical evolution, which differs from the model of progress derived from Western experience, and which imposes very different constraints upon social and economic development" (p. 7). Such a model will focus our attention on specific technologies, presented in an evolutionary framework to overcome the image of Asia as unchanging, and "should provide fresh and perhaps constructive insights into contemporary processes of change in Asia" (p. 7).

Each of the six main chapters of the book examines one or more aspect of this premise—some element in her hypothesis—in exhaustive spatial and temporal perspective, covering all of Monsoon Asia since the invention of wet rice agriculture.

Chapter 1, "The Rice Plant," describes the numerous types and varieties of rice and their economic applications, considers theories of the origins of wet rice agriculture, and includes a fascinating description of the constant processes of selection for desirable strains and traits that are inherent in the intensive handwork of rice production; something as simple as discarding weak or diseased seedlings during transplanting alters and directs the genetic pool of the future seed crop.

"Paths of Technical Development" (Chapter 2) is divided about equally between the building of wet rice fields and methods for raising their yields. In both cases Bray stresses that time, labor, and skill are as important as any mechanical or technological inputs. Machines and chemicals alone do not improve production, and may actually hinder it unless part of a total, integrated package of change.

The third chapter, "Water Control," is the longest and most dense. While acknowledging that great theoretical controversy rages around this topic, Bray skirts the fray in favor of providing a technical classification of the types of irrigation systems and their hydrological characteristics, and far too much about their historical and geographic distributions. Throughout she emphasizes that the irrigation system is the prime determinant of a field's worth and key to its improvement.

In the last three chapters, the topics do not align so neatly with their headings. Only the second half of "Rice and the Wider Economy" (Chapter 4) deals with other crops, other income sources for farmers, and the problems of monoculture. The first half of this chapter contains the author's most elaborate statement of her distinction—crucial to her thesis—between agricultural systems that are labor-intensive, using lots of people on very little land, and mechanized farming of vast areas with few personnel. Being "people-using" rather than "capital-using" is at the core of her argument regarding the nature of Asian development. This discussion continues in Chapter 5, "Development," where Bray presents her ideas concerning capital and productivity in rice production.

In her final chapter, "Peasant, Landlord, and State," the author is able to marshal all her preceding arguments into a concentrated and compelling defense of the concept of the individual family farm as the most efficient and economically "rational" mode of wet rice farming. Even when the farmers themselves do not own their own land they are better off than their counterparts in dry farming societies, Bray feels; neither absentee landlord nor specialized collective can ignore or maltreat the specialized skills of the labor-intensive wet rice producers. Some ills of exploitative tenancy and landlessness occur, she admits, thought most of these she ascribes to outside causes (Western colonialism, taxes in cash rather than in crops, etc.). "Landlessness" under her thesis requires a new definition, based not on lack of ownership but lack of usufruct. Recognizing that one of the big problems in increasing agricultural yields in wet rice areas is that the fields are usually too small for "economies of scale," she investigates several ways to increase field size (group farming, collectives), but rejects them in favor of the individual family farm as being socially and economically the most "rational" use of the land and its people.

This book has some serious problems. The most frequent, pervasive, and troubling is that of detail—there is always far too much or far too little. This stems from the fact that this book is basically a journal article written by an encyclopedist. Bray obviously has vast knowledge of rice agriculture and she can't help using it even when it is unproductive or counterproductive to do so. There are too many more examples from too many places and too many eras than are necessary to illustrate/demonstrate each theoretical point, yet in no case are we told as much about *that* example as we should know to fully appreciate its impact. (The analysis of ancient Angkor in Chapter 3, and mention of the Demas cf. Bimas development schemes in Indonesia in Chapter 5 are two major instances of this.) To use the analogy of wet rice planting, Francesca Bray tends to "broadcast" her seeds of information when she should be intensively nurturing a small number of perfectly chosen examples.

From this follows the question: to whom is this book addressed and for what purpose? It seems to be directed toward—and is certainly of great relevance to—technicians and bureaucrats of the international development agencies: I.M.F., World Bank, and the like. They seem most likely to be the "Eurocentric" guilty parties who launched her premise. They also seem least likely to read this particular book, primarily because it overwhelms with its detail rather than enlightening the reader with efficient, tightly-written, and skillfully-crafted illustrations. As an Asian scholar, I personally will use this book mainly as an index/annotated bibliography to find specific, detailed, encyclopedic examples of rice agriculture, including studies by Bray herself, as I need them in my own research. For ethnoscientists who want a book that tells not only *how* rice is grown, but what it *means* in the minds of those who grow it, try Lucian Hanks's *Rice and Man* (Chicago: Aldine, 1972) instead.

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SURVEY OF VERTEBRATE REMAINS FROM PREHISTORIC SITES IN THE SAVANNAH RIVER VALLEY

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ABSTRACT.—Faunal remains from six sites located in the Savannah River Valley were examined. While these samples represent four of the physiographic regions of the valley, temporal coverage for each region is limited. The samples appear to show a similar pattern of faunal use throughout the valley, except in the upper reaches of the Savannah estuary. These similarities seem to transcend temporal parameters. Analysis of the piedmont data suggests a subsistence strategy which incorporated a wide variety of vertebrate resources into the diet, but emphasized venison as the primary source of meat. Riverine resources were also extensively used. In the estuarine end of the river valley, however, subsistence efforts produced a diverse faunal assemblage in which fish and mammals other than deer were important. It appears unwise to extend patterns derived from piedmont sites to coastal sites and vice versa. A survey of these data clearly demonstrates that much research remains to be done in the valley in order to understand subsistence patterns.

INTRODUCTION

In spite of intensive archaeological interest in human occupation of the Savannah River Valley, remarkably little data other than site location are available for study of human subsistence strategies. One factor contributing to this situation is that much of the Piedmont portion of the river valley was flooded after construction of hydroelectric installations and prior to development of subsistence models requiring controlled collection of biological samples. Further, such important sites as Stallings Island and the Irene Mound were excavated at a time when research was primarily focused on chronological problems. Hence, many of the well-known sites from the valley were either lost or excavated before subsistence questions were clearly formulated and a strategy for answering them with biological data developed. Additionally, preservation of plant and animal remains has been poor at many sites. Hence not only are there no complete

biological collections and few faunal ones available for study, it is unlikely that this situation will improve substantially in the future. It is therefore important to take advantage of every opportunity to examine well preserved biological collections which have been excavated from the valley, and to encourage maximum recovery of biological materials from excavations in the future.

A review of the literature has produced six quantified reports of vertebrate fauna from prehistoric sites in the Savannah River Valley (Fig. 1)—the Second Refuge site (38JA61), Rabbit Mount (38AL16), G.S. Lewis Site (38AK228), Beaverdam Creek (9EB85), Clyde Gulley (9EB387), and Rucker's Bottom (9EB91). The samples represent four of the physiographic regions of the valley and several temporal affiliations. It therefore may be possible to summarize our understanding of subsistence strategies in the valley using faunal evidence much as has been done using site location (e.g. Stoltman 1974; Hanson *et al.* 1978; Brooks *et al.* 1986).

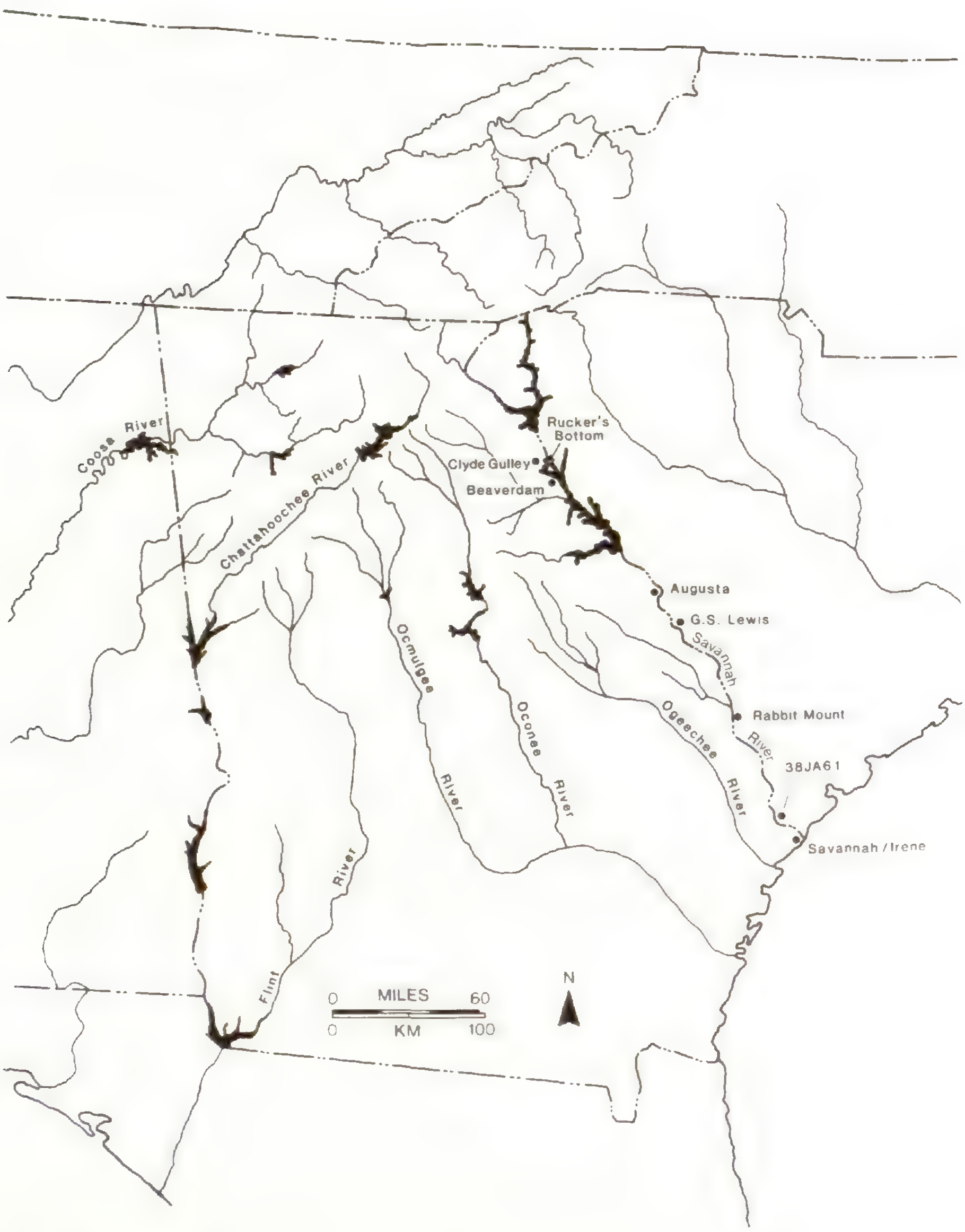

Figure 1.—Savannah River Valley.

When the six vertebrate samples are compared it can be seen that a number of factors hinder reliable comparisons, one of which is different recovery methods. At least 1/8-inch mesh was used at five of these sites but no screen was used at Rabbit Mount. Differences in the faunal assemblages between Rabbit Mount and the other collections could be attributed to differences in recovery techniques. However, we are fortunate that techniques were used which generally recovered the full size range of exploited vertebrates. It also appears that while some of the materials have been recovered from village middens, others have been excavated from ceremonial mounds and may represent secondary deposits brought in from elsewhere.

More important, however, is the lack of data for all of the temporal and physiographic settings in the valley. Although the samples available for study are from sites with well documented physiographic context and most samples are dated to within a 200 year time frame, evidence for each time period is not represented by equal sized samples from each of the ecological zones. This characteristic of the data is a source of error for interpreting Savannah River Valley subsistence patterns which is important to remember, and the need for replication for temporal and physiographic settings is not satisfied. Since the smaller temporal components at these multi-component sites are highly variable in volume of faunal remains, and since the goal here is a synthesis, the materials from each site are best used as examples of subsistence during gross chronological periods such as Mississippian instead of finer ones such as the Lamar phase, although the finer chronological units will be used here when possible.

In the review which follows, Minimum Numbers of Individuals (MNI) is used as the comparative unit because it is the most consistently reported for the sites from the valley. MNI is subject to a number of problems, among which is sample size bias. Samples of less than 200 individuals are often considered too small for reliable interpretations (Wing and Brown 1979:119), because the species list is too short and the abundance of one species in relationship to others is probably somewhat inaccurate. The criterion of 200 individuals was developed using primarily Caribbean coastal samples. Sites characterized by a marine oriented subsistence base tend to be more diverse than continental sites. It is probable that larger collections are needed for coastal sites in order to assess the extent of that diversity. Faunal collections from sites in less diverse environments, such as those in which most of the sites reviewed here were located, may be naturally less diverse and hence smaller samples may be adequate. In order to test this proposition several samples which are unquestionably more than adequate would be needed, and these are not available for the valley. The number of fragments per taxon (NISP) is also provided.

The abundance of at least two of the identified taxa is probably underestimated by MNI. Both turtles and gar were identified primarily from unpaired elements: carapace and plastron fragments for turtles and scales for gars. Considering the high bone count for these taxa, the number of individuals estimated in these collections seems too low.

When comparisons such as this are made, differences in sample size are an important source of bias. Comparing either MNI or NISP demonstrates a wide variation in sample size among the collections. Analysis of samples as small as that for the Archaic component at Rucker's Bottom is questionable (MNI = 7, NISP = 78; Scott 1985) and comparing it to larger samples is even more risky. While temporal components are obviously important, the uneven faunal representation from these different time periods makes analysis of subsistence through time in the river valley sketchy.

In order to discuss diet, biomass was determined for the four larger samples. In these cases, biomass was determined using allometric relationships and linear regression (Reitz and Cordier 1983; Reitz *et al.* 1987; Wing and Brown 1979:127-135). The formulae used are presented in Table 1.

Table 1.—*Allometric Values Used in Study*[a].

Faunal Category	N	Y-Intercept	Slope	r^2
	Bone Weight to Body Weight			
Mammal	97	1.12	0.90	0.94
Bird	307	1.04	0.91	0.97
Alligator	3	0.91	0.89	0.89
Turtle	26	0.51	0.67	0.55
Snake	26	1.17	1.01	0.97
Osteichthyes	393	0.90	0.81	0.80
Non-Perciformes	119	0.85	0.79	0.88
Siluriformes	36	1.15	0.95	0.87
Centrarchidae	38	0.76	0.84	0.80

[a]Key to abbreviations: Formula is $Y = aX^b$; where y is biomass, x is bone weight, a is the Y-intercept; and b is the slope; N is the number of observations. (Reitz and Cordier 1983; Reitz *et al.* 1987; Wing and Brown 1979.)

One methods by which variety and degree of specialization in the samples can be compared is to calculate the diversity and equitability of the species identified from each site (Hardesty 1975). Diversity measures the number of species used at the site. Equitability measures the degree of dependence on the utilized resources and the effective variety of species used at the site based upon the even, or uneven, use of individual species. Use of these indices allows discussion of food habits in terms of the variety of animals used at the site (richness or diversity) and the equitability (evenness) with which those species were utilized.

Diversity can be calculated through use of the Shannon-Weaver Index. The formula for the index is:

$$H' = -p_i \log_e P_i$$

where P_i is the number of *ith* species divided by the sample size (Pielou 1966; Shannon and Weaver 1949:14). P_i is actually the evenness component since the Shannon-Weaver Index measures both how many species were used and how much each was utilized. Diversity can be calculated both for MNI and for biomass.

Equitability is calculated using the formula:

$$E = H' / H \max$$

where *H'* is the Diversity Index and *H max* is the natural log of the number of observed species (Pielou 1966; Sheldon 1969). Equitability can be determined for both MNI and biomass.

Interpretation of the indices can be difficult. Basically diversity increases as both the number of species and the equitability of species abundance increases. A diversity index of 4.99 is the highest possible value. A sample with many species identified and in which the number of individuals slowly declines from most abundant to least abundant will be high in diversity. Diversity can be increased by adding a new taxon to the list; but if another individual of an already present taxon is added, diversity is decreased.

A low diversity can be obtained either by having few species, or by having a low equitability, where one species is considerably more abundant than another. Low equitability indicates that one species was more heavily used than other species in the sample. High equitability, approaching 1.0, indicates an even distribution of species in the sample following a normal pattern where there are a few abundant species, a moderate number of common ones, and many rare ones. It is important to note that diversity and equitability are dependent upon sample size (Grayson 1981:82-85, 1984) and are not more reliable than the derived data (MNI, bone weight, bone count, biomass) used in generating the indices.

Reference is occasionally made to season of occupation, which was estimated in several ways. One method was based on the seasonal habits of the taxa identified. None of the animals identified from the sites hibemate during the winter, although some may become scarce or less active. Another method was based on the age of deer evidenced by the degree of epiphysial fusion for selected elements. Along the area of growth the shaft and epiphysis are not fused. Growth is complete and fusion occurs in a regular temporal sequence (Gilbert 1980; Schmid 1972; Silver 1963), although environmental factors influence the age at which fusion is complete. Age was also determined by the eruption sequence and degree of wear of teeth.

The species identified were summarized into faunal categories. Although mammals are primary terrestrial resources, many identified in these collections are generally associated with damp, bottom-land conditions. Others, such as beaver, are primarily found in aquatic situations, although they may be found on land. Wild birds include the turkey and other taxa which are primarily terrestrial as well as ducks, which are primarily aquatic. With the exception of the box turtle, all of the turtles are aquatic resources. However, aquatic turtles may also be found on land during their nesting seasons and box turtles are found in low-lying areas. The summary categories also include taxa such as dogs and shrews which may have been commensal. While these animals could have been used for food as noted for other collections, there are no modifications to the bones from these sites which would clearly suggest that they represent food remains.

THE SAVANNAH RIVER VALLEY

The Savannah River flows southeast from its origin in the Blue Ridge Mountains in South Carolina to Georgia then to the Atlantic Ocean near the city of Savannah, Georgia, traversing Mountain, Piedmont, and Coastal Plain physiographic regions. The Mountain Province has a variable topography of rugged mountains, small plateaus, and narrow, winding valleys with elevations between 609 and 1220 m above sea level (Cooley 1974:3-7). The Piedmont Province, an area of rolling hills, ranges in elevation from 609 m in the foothills to about 122 m at the Fall Line. The Fall Line represents the Atlantic shoreline during its last transgression and the Coastal Plain is an emerged portion of the Continental Shelf. The Coastal Plain is generally flat, although the Savannah River valley is characterized by a series of high bluffs which rise from 16 km south of Augusta, Georgia, to about 40 km north of Savannah (Cooley 1974:5). The Upper Coastal Plain is a region of sand hills with unconsolidated sedimentary Miocene deposits of marine origin. The Lower Coastal Plain is a Pleistocene terrace of unconsolidated sands and clays. At its mouth the river forms a complex estuarine system similar to others along the Georgia and South Carolina coast. The upper reaches of that system are characterized by lower salinity ranges while the lower reaches have higher salinity ranges (Dahlberg 1975:113). Both areas are affected by tidal flow. Before entering the estuary, the river valley is narrow and deeply incised with a restricted floodplain. Meanders are present but are not common.

Human occupations in the Savannah River valley are divided into the same temporal periods as elsewhere in the Eastern United States, with temporal phases reflecting local lithic and ceramic assemblages (Stoltman 1974; Anderson *et al.* 1986). Although the sequence begins with Paleo-Indian, it is not until the Archaic Period that sites are common. The Archaic economy was based upon hunting, gathering, and fishing. Archaic sites have been found associated with large stream systems although upland areas probably were also used (Stoltman 1974; Hanson *et al.* 1981:10, 15). Early Woodland sites (referred to as Refuge Phase on the coast) were occupied between 1000 BC and 500 BC. The locations of Refuge sites appear to be similar to those of the Late Archaic, with sites on floodplain terraces and upland areas (Hanson *et al.* 1981:11). Middle Woodland (Deptford Phase on the coast) sites were occupied from about 500 BC to AD 500. Deptford sites are found along river terraces and floodplains as well as in upland areas (Hanson *et al.* 1981:13). The location of Deptford sites suggests that there may have been less use of floodplain than upland resources (Hanson *et al.* 1981:16). The Late Woodland and Mississippian Periods date from approximately AD 700 into the historic period (Anderson *et al.* 1986). Savannah I Phase sites were occupied during the Late Woodland on the coast. Above the Fall Line Savannah I sites are rare, and appear to have been occupied too late to be classified as Late Woodland. Mississippian (Savannah II Phase) sites are associates with domesticated crops. Interior sites occupied during the Mississippian are classified locally as Lamar, a phase which begins somewhat earlier than the coastal Mississippian Savannah II Phase. Mississippian sites are found on terraces and floodplains, but there is greater use of upland areas than previously.

THE COAST

No quantified faunal data are available from the lower reaches of the Savannah River estuarine system, however they are available from a site in the upper reaches of that system. (For a review of representative estuarine data see Reitz 1982). The Second Refuge Site (38JA61) lies within the Savannah National Wildlife Refuge near present-day River Mile 20, in the lower coastal plain. It was found during a survey of the wildlife refuge in 1978 (Marrinan 1979) and is named for its chronological affiliation and close association with the Refuge site reported by Waring (in Williams 1968). The site is a shell midden located on the east side of the river channel in marshy surroundings that evidence extensive alteration for waterfowl habitat management. Much of the current system of dikes and canals is based upon antebellum alterations for wet rice production. The west edge of the midden has been cut by a dredged canal.

Because of planned dike renovation, which included redreging the canal, the site was excavated by Larry Lepionka in 1979. The results of this excavation were somewhat surprising. The total depth of deposit was 2.5 meters. Invertebrate midden constituents were largely freshwater mussels and snails, but considerable estuarine fauna was present. A small sample of the 1979 vertebrate fauna (1,017 fragments) was analyzed by Marrinan (1980). Among the vertebrate fauna, the expected riverine species were present along with marine and estuarine species.

A second excavation was undertaken in 1980 (Lepionka *et al.* 1983). At this time, a more systematic collection of fauna was made using both 1/4-inch and 1/8-inch screen. The 1980 sample comprised in excess of 47,296 fragments recovered from 1 x 1 meter units excavated in 10 cm levels. Faunal material was collected in 1/4-inch and 1/8-inch screens. For this study, all of the faunal remains from the 1/4-inch screen was analyzed and a single bag from each 10 cm level was analyzed for the 1/8-inch screen.

The bulk of the deposit (Levels 12-25) was designated Refuge Phase (ca. 1000-500 BC). A radiocarbon assay of shell at the midden base was dated 1070 BC ± 115 (QC-784)

(Lepionka *et al.* 1983:38). Two levels were designated Deptford Phase (10-11) and seven levels at post-Deptford (3-9). A second date, 510 BC ± 110 (QC-785) was obtained from one meter above the midden base, approximately Level 15 (Lepionka *et al.* 1983:38). All of the materials reported here are Refuge Phase. Faunal data organized by individual levels are included in Lepionka *et al.* (1983).

The umbos of all bivalves were retained from the 1/4-inch screen in order to calculate invertebrate MNI. The most common invertebrate taxon throughout the occupation was the freshwater mussel, *Elliptio icterina* (MNI = 32,869). Oysters, *Crassostrea virginica* (MNI = 2985), were the most abundant estuarine invertebrate but were numerically less common than *Elliptio* in every level. Although fluctuations in the freshwater mussel/marine oyster ratio occur, oysters are never more than 34% of the invertebrate MNI and freshwater mussels are never less than 58% of the invertebrate MNI. Contrary to the interpretation of these data made by Brooks and his colleagues (1986) and by Claassen (1986), oysters are never the dominant invertebrate resource nor do these data suggest shifts from estuarine to freshwater regimes, particularly in the upper levels (Lepionka *et al.* 1983:231). Gastropod invertebrate MNI was based on specimens recovered in largely whole condition. Snails, *Viviparus georgianus* (MNI = 726), were present in minor quantities. Two crab fragments also were recovered.

Table 2 presents the species list for the 1980 collection. Some 61 taxa, representing at least 115 vertebrate individuals were identified in the 47,296 fragments. MNI and biomass for the sample are summarized (Tables 3 and 4). Deer was the dominant mammalian taxon, representing 17% of the 29 mammal individuals. In a consideration of biomass, the overwhelming importance of mammalian fauna is apparent (71% of the biomass for which MNI was estimated) with deer alone contributing 59% of the biomass.

Table 2.—*38JA61: 1980 Composite Species List.*

Taxon	Cnt	MNI #	MNI %	Weight (gm)	Biomass kg	Biomass %
UID Large Mammal	616			797.9	14.4962	21.7
UID Mammal	905			324.7	6.6079	9.9
Didelphis virginiana Opossum	22	5	4.4	15.4	0.3994	0.6
cf. *Sylvilagus* spp. possible Rabbit	3	1	0.9	0.5	0.0150	0.02
Sylvilagus spp. Rabbit	26	6	5.2	16.6	0.4109	0.6
UID Rodent	55	2	1.7	5.4	0.1574	0.2
cf. *Sciurus* spp. possible Squirrel	2			0.5	0.0140	0.02
Sciurus spp. Squirrel	3	1	0.9	0.5	0.0155	0.02
Castor canadensis Beaver	15	1	0.9	31.0	0.6859	1.0
cf. *Oryzomys* spp. possible Rice rat	2	1	0.9	0.3	0.0094	0.01

Table 2.—*38JA61: 1980 Composite Species List.* (continued)

Taxon	Cnt	MNI #	MNI %	Weight (gm)	Biomass kg	Biomass %
Ursus americanus Black bear	1	1	0.9	9.7	0.2032	0.3
Procyon lotor Raccoon	59	4	3.5	37.7	1.0915	1.6
Lontra canadensis Otter	2	1	0.9	2.2	0.0534	0.08
Felis rufus Bobcat	2	1	0.9	7.1	0.1577	0.2
Artiodactyla	48			315.8	5.4611	8.2
Odocoileus virginianus Deer	208	5	4.4	873.7	15.4302	23.1
UID Bird	21	3	2.6	15.4	0.2277	0.3
UID Reptile	4			0.6	0.0134	0.02
Alligator mississippiensis Alligator	2	1	0.9	2.1	0.0344	0.05
cf. Turtle	1			1.1	0.0337	0.05
UID Turtle	2754			414.8	5.3684	8.0
Chelydra serpentina Snapping turtle	5	1	0.9	3.0	0.0878	0.1
Kinosternidae Mud and Musk turtles	158	9	7.8	44.1	1.0241	1.5
Kinosternon spp. Mud turtle	4			2.2	0.0682	0.1
cf. *Sternotherus* spp. possible Musk turtle	2			2.0	0.0503	0.08
Emydidae Pond and Box turtles	130	4	3.5	66.1	1.1488	1.7
cf. *Pseudemys* spp. possible Pond turtle	16			12.2	0.2411	0.4
Pseudemys spp. Pond turtle	28			25.9	0.4856	0.7
cf. *Terrapene carolina* possible Box turtle	2			1.0	0.0390	0.06
Terrapene carolina Box turtle	14			9.7	0.1449	0.2
Trionyx ferox Softshell turtle	7	1	0.9	27.2	0.4148	0.6

Table 2.—*38JA61: 1980 Composite Species List.* (continued)

Taxon	Cnt	MNI #	MNI %	Weight (gm)	Biomass kg	Biomass %
Iguanidae Lizards	2	1	0.9	0.2	0.0026	tr
UID Snake	829			59.5	0.8477	1.3
Colubridae Non-poisonous snakes	11			1.0	0.0188	0.03
Farancia spp. Mud snake	1	1	0.9	0.6	0.0082	0.01
Nerodia spp. Water snake	5	1	0.9	0.9	0.0122	0.02
cf. Viperidae possible Pit Vipers	1	1	0.9	0.4	0.0095	0.01
Amphiumidae Salamanders	8	1	0.9	0.8	0.0105	0.02
Anura Frog/Toad	3	1	0.9	1.5	0.0205	0.03
Chondrichthyes Cartilaginous fishes	4			0.6	0.063	0.09
Selachii Sharks	6	1	0.9	0.8	0.1007	0.2
Galeocerdo cuvieri Tiger shark	1	1	0.9	0.3	0.0447	0.07
Rajiformes Skates and Rays	16	2	1.7	3.0	0.4301	0.6
UID Fish	8637			317.8	5.7717	8.6
Acipenser spp. Sturgeon	4	1	0.9	1.4	0.0447	0.07
Lepisosteus spp. Gar	813	2	1.7	57.8	1.4278	2.1
Amia calva Bowfin	789	9	7.8	59.1	1.4837	2.2
Siluriformes Catfishes	581	33	28.7	70.3	1.031	1.5
Ictaluridae Bullhead catfishes	28			8.7	0.1691	0.3
Ictalurus spp. Bullhead catfish	20			5.0	0.0992	0.2
Ariidae	29			15.0	0.276	0.4

Table 2.—*38JA61: 1980 Composite Species List.* (continued)

Taxon	Cnt	MNI #	MNI %	Weight (gm)	Biomass kg	Biomass %
Sea catfishes						
Ariopsis felis	1			0.1	0.0022	tr
Hardhead catfish						
Bagre marinus	77			8.8	0.1771	0.3
Gafftopsail catfish						
Centrarchidae	34	9	7.8	7.6	0.1404	0.2
Sunfishes						
cf. *Lepomis* spp.	3			1.1	0.022	0.03
possible Sunfish						
cf. *Micropterus* spp.	2			0.6	0.0097	0.01
possible Bass						
Micropterus spp.	2			0.2	0.0050	0.01
Bass						
Sciaenops ocellatus	1	1	0.9	0.5	0.0232	0.03
Red drum						
Mugil spp.	4	1	0.9	0.4	0.0160	0.02
Mullet						
Sphyraena barracuda	2	1	0.9	0.2	0.0080	0.01
Barracuda						
UID Vertebrate	30265			530.4		
TOTALS	47296	115		4221.0	66.8662	

Table 3.—*MNI Summaries.*

	38JA61 MNI	38JA61 %	G.S. Lewis MNI	G.S. Lewis %	Beaverdam MNI	Beaverdam %	Rucker's MNI	Rucker's %
Deer	5	4.4	34	26.4	35	21.7	137	35.8
Other Mammals	24	20.9	23	17.8	24	14.9	47	12.3
Birds	3	2.6	5	3.9	11	6.8	24	6.3
Turtles/Alligator	16	13.9	19	14.7	33	20.5	65	17.0
Snakes/Lizards	4	3.5	2	1.6	7	4.4	27	7.1
Amphibians	2	1.7	3	2.3	3	1.9	22	5.7
Sharks/Fishes	61	53.0	41	31.8	48	29.8	61	15.9
Commensal Taxa	—		2	1.6	—		—	
TOTALS	115		129		161		383	

Table 4.—*Biomass Summaries.*

	38JA61		G.S. Lewis		Beaverdam		Rucker's	
	kg	%	kg	%	kg	%	kg	%
Deer	15.4302	58.5	30.5014	85.9	36.23	86.5	48.08	86.2
Other Mammals	3.1993	12.1	1.5381	4.3	0.66	1.6	2.39	4.3
Birds	0.2277	0.9	0.4614	1.3	1.52	3.6	2.57	4.6
Turtles/Alligator	2.7099	10.3	2.3182	6.5	2.58	6.2	2.05	3.7
Snakes/Lizards	0.0325	0.1	0.0124	0.03	0.05	0.1	0.15	0.3
Amphibians	0.0310	0.1						
Sharks/Fishes	4.7503	18.0	0.4513	1.3	0.84	2.0	0.54	1.0
Commensal Taxa			0.2350	0.7				
TOTALS	26.3809		35.5178		41.88		55.78	

Avian remains were rare in the sample and none were identified to species. The MNI for birds is based on variation in fragment size rather than paired elements. Reptilian fauna was abundantly represented, particularly turtles. Turtles and alligator contributed 10% of the biomass calculated for this sample. Six turtle species were present, representing a minor range of habitats limited to the riverine environment. Sharks and fishes contributed 18% of the biomass, although cartilagenous fishes were present in only small quantities. Greatest MNI production may be seen among the bony fish with 57 individuals identified. Bony fishes such as sturgeon, bowfin, catfishes, and basses represent riverine taxa, but the collection also includes redfish, mullet, and barracuda from the estuarine environment. Redfish and barracuda are more reliable marine indicators since mullet will inhabit freshwater river systems. Barracuda is represented by teeth which had been utilized, perhaps for sawing. These remains may indicate an item of trade or a curated tool rather than subsistence activity.

Diversity and equitability of 38JA61 (Table 5) suggest that a modest range of species were present in the collection, but that most taxa were present in relatively even numbers, except for catfishes. However, the fact that catfishes contributed 29% of the individuals in the sample, reduced the collection's diversity and equitability. The prominence of venison in the collection in reflected in the low diversity and moderate equitability based on biomass. It is apparent, however, that this collection is more diverse and equitable in terms of biomass than others to be discussed in the following pages although the subsistence strategy was obviously selective in the use of available resources.

THE COASTAL PLAIN

One of the better known collections from the Savannah River Valley is located on the middle coastal plain approximately 30 miles upstream from Savannah, Georgia. Rabbit Mount (38AL15) on the river floodplain of Groton Plantation, South Carolina was occupied during the Late Archaic and Woodland Periods, and was excavated in 1964 (Stoltman 1974). The bulk of the shell midden sample is from the Late Archaic Stallings Island Phase, although Stoltman reports that few differences are found above, within, or below the Stallings Island shell midden (1974:144). Faunal remains were recovered by troweling without screens, thereby limiting recovery of small mammals, birds, reptiles, and fish. Bone counts and elements are reported although bone weight is not.

Table 5.—*Diversity and Equitability.*

	Number of Taxa	MNI	Diversity	Equitability
38JA61	35	115	2.8310	0.7963
Lewis Site	30	129	2.7950	0.8218
Beaverdam Creek	33	161	2.8822	0.8243
Rucker's Bottom	51	383	2.8700	0.7300

	Number of Taxa	Biomass	Diversity	Equitability
38JA61	35	26.8662 kg	1.8038	0.5073
Lewis Site	29	35.5178 kg	0.7917	0.2351
Beaverdam Creek	32	41.9047 kg	0.7157	0.2065
Rucker's Bottom	48	55.7772 kg	0.7380	0.1906

The collection contained 2,148 bones and at least 66 mammalian individuals (Stoltman 1974:145). Stoltman (1974:141) estimated that 63% (66% of the fragments) of the vertebrate portion of the diet was contributed by deer. Other mammals included opossum, rabbit, beaver, muskrat, gray fox, raccoon, skunk, otter, and bobcat. These contributed 6% of the fragments in the collection. Birds contributed 5% of the fragments, with turkey and quail the only birds identified. Stoltman reported that turtles contributed 19% of the fragments, with snapping turtles being the most common turtle by bone count. Turtles also included mud or musk turtles, box turtles, pond turtles, and softshell turtles. Fishes identified were gar, bowfin, and bullhead catfish. Fishes contributed 4% of the fragments. This collection was interpreted as evidence of the importance of terrestrial vertebrate resources, although aquatic vertebrates and invertebrate resources were also used.

The G.S. Lewis site (38AK228) is located in the Savannah River Plant in the upper coastal plain and was excavated by the South Carolina Institute of Archaeology and Anthropology under the direction of Glen Hanson (1985). Deposits from the site date from the Late Archaic through Late Woodland Period. The faunal materials reported here are associated with a Woodland or Deptford Phase village. Materials were recovered with 1/4-inch and 1/8-inch screens. In calculating MNI the field specimens associated with features were analyzed as independent observations. The remaining non-feature deposits were analyzed as a single observation. When possible, elements were measured following criteria established by Driesch (1976) and these are reported elsewhere (Reitz and Frank 1985).

Analysis of the Lewis site sample indicates heavy use of deer, with a wide range of other taxa supplementing the vertebrate based portion of the diet (Tables 3, 4, and 6). Fish contributed 32%, deer 26%, and other mammals 18% of the individuals. Reptiles were the other major group of animals used, with the percentage of turtle individuals included in the collection similar to that for turtle fragments identified from Rabbit Mount. The only birds identified were turkeys. Commensal taxa identified included a dog, identified from a single tooth, and a short-tailed shrew. In terms of biomass, deer were the major contributor to the diet. All other taxa were minimal contributors by comparison. Based on biomass, turtles were the major secondary resource, followed by other mammals, turkeys, and fishes.

Table 6.—*Lewis Site: Species List.*

Taxon	Cnt	MNI #	MNI %	Weight (gm)	Biomass kg	Biomass %
UID Mammal	4964			3116.93	38.977	50.4
Didelphis virginiana Opossum	5	3	2.3	7.33	0.1753	0.2
Blarina brevicauda Short-tailed shrew	1	1	0.8	0.02	0.0008	tr
Sylvilagus spp. Rabbit	5	3	2.3	5.97	0.1313	0.2
Sciurus spp. Squirrel	4	3	2.3	0.31	0.0099	0.01
cf. *Castor canadensis* possible Beaver	1			0.50	0.0141	0.02
Castor canadensis Beaver	14	3	2.3	17.80	0.3615	0.5
Ondatra zibethicus Muskrat	1	1	0.8	1.49	0.0377	0.05
Carnivore	1			0.18	0.0056	0.01
Canis familiaris Dog	1	1	0.8	11.35	0.2342	0.3
cf. *Urocyon cinereoargenteus* possible Gray fox	1			0.79	0.0213	0.03
Urocyon cinereoargenteus Gray fox	5	3	2.3	4.52	0.1065	0.1
Ursus americanus Black bear	3	2	1.6	11.29	0.2455	0.3
cf. *Procyon lotor* possible Raccoon	2			0.62	0.0171	0.02
Procyon lotor Raccoon	15	3	2.3	14.32	0.3116	0.4
cf. *Mephitis mephitis* possible Striped skunk	1	1	0.8	0.31	0.0092	0.01
Felis concolor Cougar	2	1	0.8	6.90	0.1496	0.2
Equus spp. Horse	1			58.70		

Table 6.—*Lewis Site: Species List.*

Taxon	Cnt	MNI #	MNI %	Weight (gm)	Biomass kg	Biomass %
cf. *Odocoileus virginianus* possible Deer	11			23.17	0.4451	0.6
Odocoileus virginianus Deer	807	34	26.4	2289.29	30.5014	39.4
UID Bird	45			20.32	0.3451	0.5
Galliformes	1			0.34	0.0076	0.01
cf. *Meleagris gallopavo* possible Turkey	5			3.94	0.0773	0.1
Meleagris gallopavo Turkey	12	5	3.9	28.02	0.4614	0.6
Alligator mississippiensis Alligator	1	1	0.8	15.49	0.1991	0.3
UID Turtle	456			118.01	1.2435	1.6
Kinosternidae Mud and Musk turtles	37	8	6.2	11.20	0.2579	0.3
Emydidae Pond and Box turtles	4			4.40	0.0853	0.1
Pseudemys spp. Pond turtle	44	5	3.9	61.18	0.5985	0.8
Terrapene carolina Box turtle	27	1	0.8	26.35	0.2831	0.4
Trionyx spp. Softshell turtle	131	4	3.1	116.62	0.9796	1.3
UID Snake	7			0.73	0.0098	0.01
Colubridae Non-poisonous snakes	7	2	1.6	0.90	0.0124	0.02
UID Amphibian	1			0.15		
Anura Frog/Toad	12	3	2.3	0.24		
Odontaspis taurus Sand tiger shark	1			0.19		
Lamnidae Mackerel shark	1			0.66		
Galeocerdo cuvieri Tiger shark	2			1.90		
UID Fish	395			26.35	0.5765	0.8

Table 6.—*Lewis Site: Species List.*

Taxon	Cnt	MNI		Weight	Biomass	
		#	%	(gm)	kg	%
Acipenser spp. Sturgeon	6	2	1.6	0.94	0.0332	0.04
Lepisosteus spp. Gar	56	11	8.5	5.48	0.1632	0.2
Amia calva Bowfin	6	3	2.3	1.10	0.0376	0.05
Clupeidae Herrings	17	4	3.1	0.23	0.0113	0.01
cf. *Esox* spp. possible Pickerel	1			0.02	0.0085	0.01
Esox spp. Pickerel	1	1	0.8	0.68	0.0223	0.03
Catostomidae Suckers	1	1	0.8	0.02	0.0014	tr
cf. *Ictalurus* spp. possible Bullhead catfish	1			0.13	0.0029	tr
Ictalurus spp. Bullhead catfish	33	16	12.4	7.99	0.1529	0.2
Noturus spp. Madtom	1	1	0.8	0.04	0.0009	tr
Centrarchidae Sunfishes	9			0.63	0.0154	0.02
Micropterus spp. Bass	3	2	1.6	1.60	0.0285	0.04
UID Vertebrate				667.92		
TOTALS	7169	129		6695.56	77.3699	

The horse and the three sharks are not subsistence refuse. The horse was a Pleistocene species, although the exact species was not determined. The site is deposited on a Pleistocene formation and the element is from one of the lower strata, Level D. It could have been mixed with the archaeological materials. The sharks were identified from fossilized teeth recovered from Levels D, F, and E. These are also strata at the lower levels of the site so that the teeth may be incidental inclusions in the archaeological deposit. While it is unlikely that the horse was associated with human activity (in the absence of Paleo-Indian artifacts), the possibility remains that the sharks may have been brought to the site by human action and represent trade with coastal groups.

Based on MNI, the collection is moderately diverse, but highly equitable (Table 5), indicating that a few taxa are very abundant in the collection, the remaining taxa less

so, but relatively equal in abundance. The abundant taxa are deer, bullhead catfish, and gar. When diversity is calculated using biomass, the prominance of venison is highlighted. Biomass diversity is very low and suggests low equitability. Deer contributed most of the biomass represented in this collection. The diversity values suggest an overall adaptation which included a moderately diverse range of taxa, but emphasized a single source for most of the meat.

PIEDMONT

In the upper Savannah River Valley data are available from three sites, all excavated as a result of construction of the Richard B. Russell Reservoir. The Beaverdam Creek site (9EB85) contained an earthlodge, platform mound, and village. It was excavated by the University of Georgia under the direction of David Hally and James Rudolph (Rudolph and Hally 1985). The Beaverdam Creek site was a small ceremonial and political center occupied during the Savannah II Phase, between AD 1200 and 1300 (Anderson *et al.* 1986). The site was located on a floodplain north and east of Beaverdam Creek, less than a kilometer from the Savannah River. A 1/4-inch mesh screen was used to recover most of the materials. In addition, faunal materials were recovered by fine screen processing. For purposes of determining MNI, features were considered separate components, as were the non-feature deposits referred to as the gray ashy layer associated with the mound, the village midden, and pre-mound midden.

The collection contained 7,573 bones, in poor condition, and the remains of at least 161 individuals. (Tables 3, 4, and 7; Reitz 1985b). Contrary to expectations, the fine-screen sample contained no new species compared to the 1/4-inch materials, although 21% of the fish individuals were found in the fine-screen samples. Terrestrial mammals accounted for 35% of the individuals identified from the site with deer contributing 22% of the individuals in the collection. Although other bird taxa were identified, turkeys were the major species. The assemblage contained similar numbers of turtle and fish individuals. The taxa identified suggest warm weather activity, however the possibility of winter occupation cannot be eliminated.

Table 7.—*Beaverdam Creek Site: Species List.*

Taxon	Cnt	MNI		Weight	Biomass	
		#	%	(gm)	kg	%
UID Mammal	1784			2029.5	24.9259	34.1
Didelphis virginiana Opossum	5	2	1.2	7.1	0.1535	0.2
cf. *Sylvilagus* spp. possible Rabbit	5			2.2	0.0535	0.07
Sylvilagus spp. Rabbit	15	5	3.1	4.7	0.1049	0.1
UID Rodent	8			0.4	0.0123	0.02
Sciurus spp. Squirrel	4			0.6	0.0169	0.02
Sciurus carolinensis Grey squirrel	1	1	0.6	0.1	0.0033	tr

Table 7.—*Beaverdam Creek Site: Species List.* (continued)

Taxon	Cnt	MNI #	MNI %	Weight (gm)	Biomass kg	Biomass %
Sciurus niger Fox squirrel	3	3	1.9	0.7	0.0191	0.03
cf. *Tamias striatus* possible Chipmunk	1	1	0.6	0.1	0.0033	tr
Castor canadensis Beaver	2	1	0.6	2.1	0.0513	0.07
Peromyscus spp. New World mouse	4	4	2.5	0.3	0.0089	0.01
Ondatra zibethicus Muskrat	2	1	0.6	2.9	0.0686	0.09
Sigmodon hispidus Hispid cotton rat	25	1	0.6	0.3	0.0089	0.01
Carnivore	1			0.2	0.0062	0.01
cf. *Urocyon cinereoargenteus* possible Gray fox	2			1.4	0.0356	0.05
Urocyon cinereoargenteus Gray fox	3	2	1.2	1.6	0.0402	0.06
Procyon lotor Raccoon	1	1	0.6	0.4	0.0115	0.02
cf. *Mephitis mephitis* possible Striped skunk	1			0.2	0.0062	0.01
Mephitis mephitis Striped skunk	3	1	0.6	2.3	0.0557	0.08
Felis rufus Bobcat	1	1	0.6	5.9	0.1299	0.2
cf. *Odocoileus virginianus* possible Deer	8			5.3	0.1188	0.2
Odocoileus virginianus Deer	653	35	21.7	3075.5	36.2347	49.6
UID Bird	695			256.8	3.1819	4.4
Cathartes aura Turkey vulture	4	3	1.9	7.9	0.1330	0.2
Meleagris gallopavo Turkey	37	7	4.4	104.6	1.4052	1.9

Table 7.—*Beaverdam Creek Site: Species List.* (continued)

Taxon	Cnt	MNI #	MNI %	Weight (gm)	Biomass kg	Biomass %
Ectopistes migratorius Passenger pigeon	1	1	0.6	0.2	0.0047	0.01
UID Turtle	924			249.1	1.2751	1.8
Chelydra serpentina Snapping turtle	3	1	0.6	4.7	0.0888	0.1
Kinosternon cf. *subrubrum* Mud turtle	3	1	0.6	1.2	0.0357	0.05
Emydidae Pond and Box turtles	75			49.3	0.4305	0.6
Pseudemys spp. Pond turtle	48	7	4.4	55.7	0.4672	0.6
Terrapene carolina Box turtle	190	11	6.8	273.0	1.3560	1.9
Trionyx cf. *spiniferus* Softshell turtle	113	13	8.1	87.3	0.6318	0.9
UID Snake	33			1.6	0.0218	0.03
Colubridae Non-poisonous snakes	27	6	3.7	3.2	0.0447	0.06
Viperidae Pit vipers	1	1	0.6	0.4	0.0055	0.01
Anura Frog/Toad	4	3	1.9	0.5		
UID Fish	2522			81.9	1.0460	1.4
Lepisosteus spp. Gar	117	9	5.6	10.8	0.1983	0.3
Catostomidae Suckers	134	14	8.7	22.4	0.3516	0.5
Minytrema melanops Spotted sucker	3	2	1.2	0.3	0.0117	0.02
Ictaluridae Bullhead catfishes	86	15	9.3	10.6	0.1871	0.3
Ictalurus catus White catfish	1			0.2	0.0043	0.01
Ictalurus punctatus Channel catfish	1			0.1	0.0022	tr

Table 7.—*Beaverdam Creek Site: Species List.* (continued)

Taxon	Cnt	MNI #	MNI %	Weight (gm)	Biomass kg	Biomass %
Centrarchidae Sunfishes	5			0.1	0.0029	tr
Lepomis spp. Sunfish	7	4	2.5	0.5	0.0705	0.1
Micropterus spp. Bass	1			0.1	0.0025	tr
Micropterus salmoides Largemouth bass	4	2	1.2	0.8	0.0144	0.02
cf. *Pomoxis nigromaculatus* possible Black crappie	1	1	0.6	0.1	0.0025	tr
cf. *Aplodinotus grunniens* possible Freshwater drum	1	1	0.6	0.02	0.0022	tr
UID Vertebrate				1649.8		
TOTALS	7573	161		8017.02	73.0473	

Diversity and equitability are very similar to the Lewis Site. The MNI diversity (Table 5) suggests a strategy in which deer were the central component, supplemented by a wide range of other taxa. This pattern indicates that resources from several biotopes were included in the subsistence strategy. While deer represent exploitation of terrestrial biotopes, 47% of the individuals identified were from aquatic biotopes. Biomass diversity is very low and the equitability also is low.

The smallest of the Russell collections is from Clyde Gulley (9EB387). This small village was located at the confluence of Pickens Creek and the Savannah River and was excavated by the South Carolina Institute of Archaeology and Anthropology under the direction of William H. Marquardt and Ann Tippitt (Tippitt and Marquardt 1984). The assemblage, recovered using 1/8-inch mesh and flotation, contained 1,479 bones and 7 individuals and was dated to Early Etowah Phase, AD 900 to AD 1000 (Mississippian Period). The vertebrate materials were poorly preserved (Ruff 1982). The largest number of bones was from mammals, followed closely by turtles. Deer was the only mammalian taxon identified and contributed 14% of the fragments, as did birds. Reptiles including mud or musk turtles and a softshell turtle, contributed 43%, and fishes, which included two bullhead catfish individuals, contributed 29% of the fragments. It is interesting that reptiles formed an even larger percentage of the fragments in this small collection than in larger ones.

The third vertebrate assemblage studied from the Russell Reservoir construction area is Rucker's Bottom (9EB91; Anderson and Schuldenrein 1983; 1985). This village was located on the Savannah River 12 km upstream from Beaverdam Creek and was excavated by David Anderson and Joseph Schuldenrein, Commonwealth Associates, Inc. The majority of the materials were from Mississippian features associated with living

floors, refuse pits and small middens found in and around the village, although some Early Woodland fauna, probably with some Archaic remains intermixed, were also recovered. Only the Mississippina fauna are reported herein. Rucker's Bottom may have been a subsidiary village to Beaverdam Creek and was occupied primarily between AD 1200-1450 (Anderson *et al.* 1986). Flotation samples were not studied intensively; however, they were scanned for new species (Scott 1985). A single additional taxon not found in the 1/4-inch fraction was identified in the smaller fraction. This was a minnow (Cyprinidae), suggesting that the 1/4-inch samples can be used to interpret the vertebrate subsistence strategy, although the number of fish individuals may be under-represented. Table 8 presents data the 1/4-inch fraction only. The Mississippian sample contained 13,015 bones and at least 384 individuals (Tables 3, 4, and 8). Although the species identified from Rucker's Bottom are very similar to those from nearby Beaverdam Creek, the proportions in which those taxa were used differ.

Table 8.—*Rucker's Bottom: Mississippian Species List.*

Taxon	Cnt	MNI		Weight	Biomass	
		#	%	(gm)	kg	%
UID Large Mammal	8517			4112.8	47.1708	43.3
UID Small Mammal	313			81.7	1.3836	1.3
Didelphis virginiana Opossum	10	5	1.3	9.7	0.2033	0.2
Sylvilagus spp. Rabbit	7	2	0.5	0.8	0.0215	0.02
Sylvilagus floridanus Cottontail rabbit	5	5	1.3	1.6	0.0402	0.04
Sciurus spp. Squirrel	2			0.2	0.0062	0.01
Sciurus carolinensis Grey squirrel	6	5	1.3	1.2	0.0310	0.03
Sciurus niger Fox squirrel	6	5	1.3	2.1	0.0513	0.05
Marmota monax Woodchuck	1	1	0.3	0.7	0.0191	0.02
Castor canadensis Beaver	4	3	0.8	6.3	0.1378	0.1
Cricetidae New World mice	5	1	0.3	0.3	0.0089	0.01
Peromyscus spp. New World mouse	7	2	0.5	0.3	0.0089	0.01
Carnivore	5			2.9	0.0686	0.06
Canis niger Wolf	2	2	0.5	1.8	0.0446	0.04
Urocyon cinereoargenteus Grey fox	3	2	0.5	0.8	0.0215	0.02
Ursus americanus Black bear	9	5	1.3	87.4	1.4701	1.4
Procyon lotor Raccoon	10	6	1.6	9.7	0.2033	0.2

Table 8.—*Rucker's Bottom: Mississippian Species List.* (continued)

Taxon	Cnt	MNI		Weight	Biomass	
		#	%	(gm)	kg	%
Mephitis mephitis Striped skunk	14	2	0.5	4.3	0.0978	0.09
Felis rufus Bobcat	1	1	0.3	1.3	0.0333	0.03
Odocoileus virginianus Deer	1335	137	35.8	4210.8	48.076	44.2
UID Large Bird	664			196.8	2.4978	2.3
UID Small/Medium Bird	102			16.8	0.2661	0.2
Meleagris gallopavo Turkey	66	22	5.7	201.7	2.5544	2.4
Ectopistes migratorius Passenger pigeon	2	2	0.5	0.5	0.0109	0.01
UID Turtle	1332			301.8	1.4501	1.3
Chelydra serpentina Snapping turtle	8	6	1.6	13.8	0.1835	0.2
Kinosternidae Mud and Musk turtles	14	4	1.0	4.1	0.0814	0.07
Kinosternon subrubrum Mud turtle	3	1	0.3	1.3	0.0377	0.03
Sternotherus spp. Musk turtle	1	1	0.3	1.1	0.0337	0.03
Emydidae Pond and Box turtles	10	1	0.3	7.5	0.1220	0.1
Pseudemys spp. Pond turtle	29	12	3.1	25.1	0.274	0.3
Pseudemys concinna/ floridana Pond turtle	1	1	0.3	1.5	0.0415	0.04
Terrapene carolina Box turtle	109	32	8.4	192.8	1.074	1.0
Trionyx spp. Softshell turtle	39	7	1.8	16.3	0.2052	0.2
UID Snake	7			1.0	0.0138	0.01
Colubridae Non-poisonous snakes	11	4	1.0	2.1	0.0292	0.03
Coluber/Masticophis spp. Racer/coachwhip	11	9	2.3	1.7	0.0236	0.02
Lampropeltis/Elaphe spp. Corn/Rat/King snakes	14	7	1.8	3.6	0.0503	0.05
Nerodia spp. Water snake	1	1	0.3	0.2	0.0027	tr
Viperidae Pit vipers	9	6	1.6	3.5	0.0489	0.04
Anura Frog/Toad	14	2	0.5	1.9		

Table 8.—*Rucker's Bottom: Mississippian Species List.* (continued)

Taxon	Cnt	MNI #	MNI %	Weight (gm)	Biomass kg	Biomass %
Rana spp. Frog	3	3	0.8	0.6		
Bufo spp. Toad	38	17	4.4	3.0		
UID Fish	154			13.9	0.2488	0.2
cf. *Acipenser* spp. possible Sturgeon	5	4	1.0	0.5	0.0175	0.02
Lepisosteus osseus Longnose gar	15	7	1.8	2.0	0.0522	0.05
Esox spp. Pickerel	1	1	0.3	0.1	0.0049	tr
Catostomidae Suckers	26	8	2.1	4.3	0.0956	0.09
Minytrema melanops Spotted sucker	6	6	1.6	1.8	0.0480	0.04
Moxostoma spp. Redhorse	18	12	3.1	3.2	0.0757	0.07
Ictaluridae Bullhead catfishes	7	1	0.3	3.1	0.0585	0.05
Ictalurus brunneus Snail bullhead	6	4	1.0	2.8	0.0531	0.05
Ictalurus catus White catfish	6	5	1.3	0.7	0.0142	0.01
Ictalurus natalis Yellow bullhead	1	1	0.3	0.2	0.0043	tr
Ictalurus punctatus Channel catfish	1	1	0.3	0.4	0.0084	0.01
Centrarchidae Sunfishes	6-	1	0.3	0.8	0.0144	0.01
Lepomis spp. Sunfish	5	4	1.0	0.6	0.0113	0.01
Micropterus spp. Bass	6	4	1.0	5.3	0.0705	0.06
Pomoxis spp. Crappie	1	1	0.3	0.2	0.0045	tr
Perca flavescens White perch	1	1	0.3	0.1	0.0025	tr
UID Vertebrate				726.4		
TOTALS	13015	383		10301.8	108.883	

The Rucker's Bottom materials appear to be very similar in diversity to those from Lewis and Beaverdam Creek (Table 5). The Rucker's Bottom sample has a very low MNI diversity and a low equitability. Although deer, toads, and box turtles are major components, most of the other taxa were used more or less evenly, with the other species represented by 1 to 6 individuals. In terms of biomass, the Rucker's Bottom sample is slightly more diverse, but even less equitable that Beaverdam.

When comparing the early Mississippian materials from Beaverdam Creek with early Mississippian materials from Rucker's Bottom, it was found that the two samples were very similar (Scott 1985:661), while the later Mississippian materials from Rucker's Bottom were quite dissimilar to the early Mississippian remains from either site. Comparing bone weight, the major difference between the early Mississippian samples from Rucker's Bottom and the material from Beaverdam Creek is that fish were more abundant in the deposits from Beaverdam Creek (Table 3), possibly because of the inclusion of flotation materials in the Beaverdam species list. Such fine-screened materials usually contain larger quantities of fish than found in 1/4-inch screened samples, and 21% of the fish individuals in the Beaverdam collection were from the fine-screened samples. The Mississippian materials provide evidence for a warm season occupation; evidence for a winter occupation could not be demonstrated. The later Mississippian Rucker's Bottom assemblage appears to have been the result of a winter occupation. It differs from the early Mississippian sample from Rucker's Bottom and that from Beaverdam Creek in that large mammal bones contribute an overwhelming percent of the Rucker's Bottom sample.

DISCUSSION

Comparison of the vertebrate data from these sites shows more similarities than dissimilarities. In spite of the collections having had inconsistent sample characteristics, the subsistence strategy reflected by the noncoastal samples appears consistent, regardless of whether the sites are above or below the Fall Line. This is a surprising result since it could be expected that the distinct physiographic zones represented by the coastal plain and the piedmont sections of the river valley would elicit distinctive subsistence responses. The samples are also from a series of different temporal units, some of which are associated with horticulture and others of which are not. It was anticipated that pre-Mississippian vertebrate exploitation would differ substantially from Mississippian vertebrate exploitation if only because of the time and labor required for horiticultural activities. Perhaps this was not the case. It must also be acknowledged that these similarities may be due to the gross level of temporal analysis. It is probable that by lumping large temporal units some artificial homogeneity has been created.

The only collection which appears dissimilar to the others is 38JA61. The location of this site in the upper reaches of the Savannah esuarine system accounts for the presence of estuarine taxa in the collection. The unique nature of estuarine assemblages has been discussed previously (Reitz 1985a). The collection from 38JA61 represents the special way in which people adapted to the riches of those systems by using large numbers of fishes. Interestingly, however, even 38JA61 seems to contain more freshwater turtles than sites located in the lower reaches of other estuaries and on the barrier islands.

The most typical characteristic of the non-coastal samples is the high presence of deer in combination with a high use of turtles. Deer was the predominant taxon, followed by either softshell turtles, snapping turtles, or pond turtles. There may be some evidence that more turtles, and birds, were used than other taxa at sites above the Fall Line. There is also some evidence to suggest that deer are more abundant in Mississippian deposits than in earlier ones.

In sharp contrast to 38JA61 and other coastal sites, very few fish taxa appear to have been exploited at sites from the coastal plain into the piedmont. Few anadromous individuals were identified. Perhaps such fishes were not used, or depositional processes have mitigated against their recovery. Interestingly, the only members of the herring family for which measurements are avilable indicate that the individuals which may have been used were at the small end of the size range rather than large adults (Reitz and Frank 1985).

Birds, other than turkeys, are virtually absent from the collections. Only two passenger pigeons have been identified, suggesting either that they were not exploited in the numbers historically reported, or that the sites were not occupied during months when these birds were present. The Savannah River Valley is within the former winter range of passenger pigeons, and they also nested along the coast (Schorger 1955:264, 269), so the latter explanation seems unlikely.

Interestingly, the two sites which are most similar to one another chronologically and geographically (Beaverdam and Rucker's Bottom) have faunal collections which reflect dissimilar amounts of deer, turtles, and fish. This difference is most apparent in MNI and in fragment count and might be accounted for because of different activities at the two sites, one being a ceremonial center and the other being residential in nature. The earthlodge, and later the mound at the Beaverdam Creek site, may have been associated with special functions which affected the subsistence effort. It is also possible that the differences reflect the fact that the fine-screened portion of the Beaverdam sample was quantified and contained more fish while the fine-screened sample from Rucker's Bottom was not quantified. However, the Rucker's Bottom fine-screen component appears not to have had the number of fish found at Beaverdam.

In terms of technologies and habitats involved, it appears that a diverse number of methods and locations were used. Many of the mammals are crepuscular or noctural and would best have been captured with traps rather than being actively hunted. Many of the turtles, such as the box turtles, could have been collected in conjunction with other activities or caught with fish hooks. The pond turtles could also have been captured in traps hung below logs. The fish could have been captured using hook and line (either hand-held or set), traps, weirs, spears, poisons in quiet waters, or nets. A variety of water conditions probably were exploited. These included quiet backwaters of the main river for animals such as bowfin, gar, and many sunfishes, and small rivers with some current present for pickerels and suckers.

It is interesting that the diversity and equitability values for Beaverdam Creek, the Lewis Site, and Rucker's Bottom are so similar. The three collections are all moderately diverse and highly equitable in terms of MNI but have low biomass diversity and equitability. This pattern suggests that the subsistence strategies developed on the upper coastal plain and the piedmont were similar, in spite of geographical and temporal differences. The river itself, transecting these other geographical subdivisions, was the significant biotope in the development of subsistence strategies in the valley. Strategies were designed to take advantage of the riverine resources available to supplement deer, which could also be taken along the river's edge.

Other factors played a role in subsistence strategies. Use of plant resources, primarily domesticated ones, changed through time. It is also probable that interactions among humans living in the valley and those living outside of it changed with time. However, within the valley it appears that full advantage was taken of aquatic resources to supplement a diet in which venison figured prominently throughout the time periods represented in this study. Even at 38JA61, the strategy appears to have been designed to obtain a variety of easily captured lowland and riverine/estuarine species to supplement deer.

CONCLUSION

A survey such as this one is of necessity too general. However, it serves to point out areas where additional attention may be focused in the future. Biological evidence for subsistence in the Savannah River Valley is limited. Based on available evidence it appears that, except for the lower coastal plain, subsistence strategies involving animals were very similar from the Late Archaic into the Mississippian Period throughout the

length of the valley. That strategy incorporated deer and a variety of riverine turtles and fishes. This review also highlights the need for special care in the recovery and study of biological remains.

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Alkaloids: Chemical and Biological Perspectives. Volume 5. Pelletier, S. William (ed.). New York: John Wiley & Sons (A Wiley-Interscience Publication). 1987. Pp. xiii + 714. $100.00.

Phytochemists are already familiar with Pelletier's earlier contributions and their high academic and practical value. This fifth volume in the series continues the high standard that we have found in the past. Dedicated to the late Sir Robert Robinson, the book is a collection of five chapters consisting of timely surveys of several topics in alkaloid studies by 10 experts from four countries:

1) The Chemistry and Biochemistry of Simple Indolizidine and Related Polyhydroxy Alkaloids and Related Alkaloids, by Elbein and Molyneaux; 2) Structure and Synthesis of Phenanthroindolizidine Alkaloids and Related Compounds, by Gellert; 3) Aporphinoid Alkaloids of the Annonaceae, by Cave, Leboeuf, and Waterman; 4) Thalictrum Alkaloids: Chemistry and Pharmacology, by Schiff; and 5) Synthesis of Cephalotaxine Alkaloids, by Hudlicky, Kwart, and Reed.

Each chapter has a comprehensive list of references. There are also two indices: a detailed subject index of 17 pages and an organism index (mostly of plants) of 6 pages.

This publication will be of interest to specialists because of its timeliness, the authentic material presented, and the roster of experts who have contributed to it.

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The First Resource: Wild Species in the North American Economy. Prescott-Allen, C. and R. Prescott-Allen. New Haven: Yale University Press. Pp. xv + 529, 33 figs., 128 tables. $62.00.

There has never before been a treatment published that is so full of statistical material on what has generally been considered a flora that has given little to the benefit of human affairs. This work is so thoroughly detailed and widely-inclusive that it is not possible to review here all of the statistical data that support the major premises of the monograph.

Sponsored by the World Wildlife Foundation, it discusses the many aspects of the use of the North American floras from the point of view of "wild" plants that enter or can enter into the economy of the region. While several minor treatments of this general aspect of economic botany have appeared, I know of no other volume that packs so much convincing statistical information into one contribution.

The book is divided into 12 sections: 1) Wildlife as a Resource; 2) Logging; 3) Fishing; 4) Trapping and Collecting; 5) Medicine; 6) Food and Industrial Products; 7) New Domesticates; 8) Wild Genetic Resources; 9) Pollination and Pest Control; 10) Recreational Fishing, Hunting and Nonconsumptive Wildlife Use; 11) Toward a Biogeography of Wildlife Use; and 12) Conclusion. There are several valuable appendices, notes, references, and a very adequate index.

The First Resource will be a valued addition for any student, teacher, researcher, or reference personnel in North America. It is a masterful treatment of a neglected topic.

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RECENT DOCTORAL DISSERTATIONS OF INTEREST TO ETHNOBIOLOGISTS: FALL 1986 - FALL 1987

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This is the fifth in an annual series of bibliographies listing selected dissertations drawn from the pages of Dissertation Abstracts (D.A.) (see also Hays 1983; Laferrière 1984, 1985; Laferrière and Hays 1986). All listings were constructed by scanning the titles and abstracts published in D.A. and making subjective decisions as to which ones might be relevant to work in ethnobiology or related disciplines such as ecological anthropology.

Dates covered in this edition include: Volume A (Humanities and Social Sciences): November 1986-August 1987; Volume B (Sciences and Engineering): November 1986-August 1987; Volume C (European Dissertations): Fall 1986-Summer 1987. It should be noted that these are the dates for the issues of D.A. in which the abstracts appear, rather than the dates of acceptance of the dissertations themselves. A few older dissertations overlooked in previous listings have also been incorporated.

Included in the current list are dissertations categorized in D.A. under Agricultural Chemistry, Agricultural Economics, Agriculture, Agronomy, American Studies, Analytical Chemistry, Animal Culture & Nutrition, Anthropology, Biology, Botany, Ecology, Entomology, Environmental Science, Food Science & Technology, Forestry & Wildlife, General Chemistry, Genetics, Geography, Language, Linguistics, Nursing, Nutrition, Organic Chemistry, Palaeoecology, Paleontology, Palynology, Pharmacology, Pharmacy, Public Health, Range Management, Sociology, and Zoology. The compilers attempted to be as inclusive as possible, but some appropriate dissertations may have been overlooked. Comments and suggestions would be welcome for items to include in next year's edition.

The dissertations are listed alphabetically by author, along with the year of acceptance, title, institution, length, advisor or major professor, number(s) of page(s) in D.A. where the abstract may be found, University Microfilms order number, and the ISBN number when this information is given.

Those dissertations with UMI order numbers may be purchased from University Microfilm International, P.O. Box 1764, Ann Arbor, MI 48106, either on microfilm or published by microfilm xerography. Current prices may be obtained by calling 313-761-4700 or 800-521-3042 (in Canada, 800-268-6090).

Dissertations from outside the United States are not now available through UMI, although some of these may be obtainable from the accepting institutions or through interlibrary loan. Relatively few European universities participate in the D.A. abstracting

service; hence those European dissertations listed represent a small proportion of the total. Abstracts of all dissertations listed below are, however, published in D.A., in English except as noted.

ABBAS, IBRAHIM RASHEED. (1987) Physiochemical properties of tepary bean starch. University of Arizona, 171 pp. Director: James W. Berry. D.A. 48(1):13-B. Order no. DA8709886.

ABUSUWAR, AWAD OSMAN MOHMED. (1986) Land imprinting as an effective way of soil surface manipulation to revegetate arid lands. University of Arizona, 132 pp. Director: M. H. Schonhorst. D.A. 47(5):1797-B. Order no. DA8615819.

ACKERLY, NEAL W. (1986) Archaic adaptive strategies in the Middle Gila River Basin. Arizona State University, 2 volumes, 736 pp. Chairperson: Geoffrey Clark. D.A. 47(5):1786-A. Order no. DA8616448.

ALEXANDER, LEE. (1986) Nonindustrial private forest landowner relations to wildlife in New England. Yale University, 228 pp. D.A. 47(8):3187-B. Order no. DA8628721.

ALVERSON, WILLIAM SURPRISON. (1986) *Quararibea* Aubl. S.L. (Bombaceae) in Mexico, Central America, and the Antilles: a taxonomic study. University of Wisconsin-Madison, 488 pp. Supervisor: Hugh H. Iltis. D.A. 48(1):33-B. Order no. DA8703816.

BALABANIAN, O. (1979) Les exploitation et les problèmes de l'agriculture en Estremadure espagnole et dans le Haut-Alentejo. [Agricultural exploitation in Spanish Estremadura and Upper Alentejo.] Université de Clermont-Ferand, 1,066 pp. D.A. 47(4):851-C. Dissertation in French.

BAMFORTH, DOUGLAS BRIAN. (1986) Predictability, productivity, and patchiness: ecology and human organization on the Great Plains. University of California, Santa Barbara, 378 pp. Chair: Michael Jochim, D.A. 48(1):162-A. Order no. DA8709097.

BAUMGARTNER, S. (1986) Untersuchungen an der kohlenhydratfraktion von johannisbrot, *Ceratonia siliqua* L. [Studies on the carbohydrate fraction of carob, *Ceratonia siliqua* L.] Eidgenössische Technische Hochschule Zürich, 87 pp. D.A. 48(2):292-C. Published by the Swiss Federal Institute of Technology, Department of Food Science, CH-8092 Zürich-ETH, Switzerland. Dissertation in German.

BAXTER, PAUL WILLIAM. (1986) Archaic upland adaptations in the Central Oregon Cascades. University of Oregon, 234 pp. Advisor: C. Melvin Aikens. D.A. 47(7): 2628-A. Order no. DA8622482.

BECKERMAN, IRA CARL. (1986) Prehistoric settlement and subsistence in Piedmont, North Carolina. The Pennsylvania State University, 927 pp. Advisor: James W. Hatch. D.A. 47(7):2628-2629-A. Order no. DA8623717.

BENCHERIFA, ABDELLATIF. (1986) Agropastoral systems in Morocco: cultural ecology of tradition and change. Clark University, 415 pp. Chief Instructor: Douglas L. Johnson. D.A. 47(8):3153-3154-A. Order no. DA8627753.

BENOIT, DIANA LYSE. (1986) Method of sampling seed banks in arable soils with special reference to *Chenopodium* spp. University of Western Ontario, Canada. Advisor: P. B. Cavers. D.A. 47(10):4023-B.

BENZ, BRUCE F. (1986) Taxonomy and evolution of Mexican maize. University of Wisconsin-Madison, 493 pp. Supervisor: Hugh H. Iltis. D.A. 48(2):329-B. Order no. DA8703819.

BERKOWITZ, ALAN ROBERT. (1986) Mechanisms of plant interactions in mixtures and monocultures of corn and sunflower, with particular reference to water. Cornell University, 280 pp. D.A. 47(5):1841-B. Order no. DA8607268.

BERNSTEIN, BRUCE ALAN. (1986) Migration, health, and nutrition: Haitians in Immokalee, a South Florida farmworker town. University of Connecticut, 399 pp. Major Advisor: Pertti J. Pelto. D.A. 48(1):163-A. Order no. DA 8710260.

BOCEK, BARBARA ROSE. (1987) Hunter-gatherer ecology and settlement mobility along San Francisquito Creek. Stanford University, 412 pp. Advisor: John W. Rick. D.A. 47(12):4425-A. Order no. DA8707635.

BODNER, CONNIE COX. (1986). On the evolution of agriculture in Central Bontoc. University of Missouri-Columbia, 674 pp. Co-Supervisors: Robert F. G. Spier, Ralph M. Rowlett. D.A. 47(10):3793-A. Order no. DA8701365.

BOHS, LYNN ALLISON. (1986) The biology and taxonomy of *Cyphomandra* (Solanaceae). Harvard University, 319 pp. D.A. 47(6):2280-B. Order no. DA8620561.

BOUEY, PAUL DOUGLAS. (1986) The intensification of hunter-gatherer economies: archaeological indicators of change and complexity in the North Coast Ranges of California. University of California, Davis, 256 pp. D.A. 47(10):3794-A. Order no. DA8701672.

BOURQUE, BRUCE JOSEPH. (1971) Prehistory of the Central Maine Coast. Harvard University, 443 pp. D.A. 47(10):3794-A. Order no. DA8702372.

BREDENKAMP, MARTIN WILLIAM. (1986) The isolation, structure, and chemistry of the major pyrrolizidine alkaloids of *Senecio latifolius* DC. University of Pretoria, South Africa. Promoter: A. Wiechers. D.A. 48(1):135-B.

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BUGG, ROBERT LYMAN. (1986) The use of a nectar-bearing weed *[Polygonum aviculare]* to enhance biological control. University of California at Davis, 152 pp. D.A. 47(7):2744-B. Order no. DA8621480.

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BURKE, GEORGINE SEVERSON. (1986) Sociocultural determinants of nutrient intake and arterial blood pressure among Cambodian refugees in Utah. University of Connecticut, 230 pp. Major Advisor: Gretel H. Pelto. D.A. 48(1):166-A. Order no. DA8710261.

BURLEIGH, ELIZABETH. (1986) The pattern of childhood malnutrition in San Jose Poaquil, Guatemala. University of California, Los Angeles, 274 pp. Chair: Allen Johnson. D.A. 47(6):2212-A. Order no. DA8621032.

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CAMINO, LINDA ANNE. (1986) Ethnomedical illnesses and non-orthodox healing practices in a Black neighborhood in the American South: how they work and what they mean. University of Virginia, 358 pp. D.A. 48(1):161-A. Order no. DA8705712.

CAPUANO, THOMAS MICHAEL. (1986) Agricultural terminology in Spanish Medieval and Renaissance treatises. State University of New York at Albany, 317 pp. Director: Luis Jenaro-MacLennan. D.A. 47(8):3020-A. Order no. DA8625998.

CARR, HELEN SORAYYA. (1986) Faunal utilization in a Late Preclassic Maya community at Cerros, Belize. Tulane University, 689 pp. Chairman: E. Wyllys Andrews, V. D.A. 47(7):2629-A. Order no. DA8624410.

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COTTER, JOHN VINCENT. (1986) Mosquitoes and disease in the Lower Rio Grande Valley, 1846-1986. University of Texas at Austin, 307 pp. Supervisor: Robert K. Holz. D.A. 47(12):4480-A. Order no. DA8705987.

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TENTH ANNUAL MEETING SOCIETY OF ETHNOBIOLOGY 5-7 MARCH 1987

University of Florida Gainesville, Florida

CONFERENCE CHAIRPERSON: Elizabeth S. Wing

ABRIDGED MINUTES of BUSINESS MEETING

The business meeting took place on March 7 between 1655 and 1830 h., with President Steve Weber presiding. The meeting was open to all registrants at the conference. Reports were offered by President Weber, Secretary/Treasurer Steve Emslie, Willard Van Asdall, Editor, *Journal of Ethnobiology*, and by Conference Coordinator Jan Timbrook.

President Weber described the results of the first election of officers of the Society: Amadeo Rea (President, two-year term), Elizabeth S. Wing (Vice-President/President Elect, two-year term), Cecil H. Brown (Secretary/Treasurer, two-year term), Robert A. Bye, Jr. (Board Member, four-year term), Richard Ford (Board Member, three-year term), Paul Minnis (Board Member, two-year term). The terms of office of the above began with the announcement of their election. President Weber also asked the assembled group to approve a permanent seat on the Board of Directors comparable to that held by Past Presidents for Steve Emslie in recognition of his long and tireless service to the Society. This recommendation was unanimously accepted.

Editor Van Asdall discussed criteria used in selecting members of the Editorial Board of the *Journal of Ethnobiology*. Of particular importance is that the scholarly interests of Board Members are distributed among all sub-disciplines of ethnobiology. He reported that the *Special Issue* of the journal (Vol. 6, No. 1, 1986) devoted to new directions in ethnobiology has received much attention and is a great success, so much so that another special issue along the same lines may be forthcoming in the not-so-distant future. The Editor is also considering preparation of a "guideline for papers" submitted to the journal which could be placed inside the back cover, perhaps following the example in the *Journal of Mammalogy*. He solicits ideas on the latter proposal and on other aspects of the journal's production.

Conference Coordinator Jan Timbrook reported that next year's conference (the Eleventh Annual Meeting) will be held in Mexico City, tentatively set for March 17-19, 1988, and locally organized by Robert A. Bye, Jr. The Twelth Annual Meeting (Spring 1989), for which dates are yet to be set, will be held at the University of California, Riverside and locally organized by Kathleen Truman. Bruce Smith will organize the Thirteenth Annual Meeting in Washington, D.C. some time in the Spring of 1990. Ms. Timbrook also led an open discussion concerning the possibility of providing a conference format in the future which will allow for a question-answer period after paper presentations.

REVISED DATES FOR
SOCIETY OF ETHNOBIOLOGY
ELEVENTH ANNUAL CONFERENCE
MEXICO CITY, MEXICO

Reception:
Evening of Wednesday, 9 March 1988

Oral Presentations:
Thursday and Friday, 10 and 11 March 1988

Poster Session:
Saturday, 12 March 1988

Please see page 238 for further details.

NEWS AND COMMENTS

MYSTERIOUS DISEASE OF NATIVE GUAMANIANS MAY HOLD KEY TO ALZHEIMERS

We tend to applaud the continued reliance by native peoples on their traditional foods and medicines as a triumph of the wisdom of tradition over self-righteous modern science. However, there is now strong evidence for serious health risks attributable to heavy consumption of a traditional staple—the seeds of the cycad, *Cycas circinalis L.*—by the Chamorro native people of Guam and nearby Rota in the Marianas Islands (by whom this plant is known as ***fadang***). The disorders linked to cycad consumption are amyotrophic lateral sclerosis (ALS), a progressive, fatal disorder of adults, and a related syndrome, parkinsonism-dementia (PD).

According to a report in *Science* (237:517-522, with commentary on 483-484, 31 July 1987) by P. S. Spencer et al., "Guam ALS is characterized by progressive limb weakness, spasticity, bulbar dysfunction, fasciculation, and muscle atrophy." The associated PD syndrome provides a close clinical parallel to more widespread diseases such as Huntington's chorea, Parkinson's disease, and Alzheimers. Thus a solution to the mystery of Guam ALS/PD might have broad medical ramifications.

Epidemiologists have been stumped for the past 35 years by the extraordinary incidence of this set of degenerative neurological disorders among the Chamorro, for whom death rates from ALS are 50 to 100 times those of the continental U.S. Cycad seeds have long been suspect as a cause, but an early failure to link cycad ingestion to such conditions in laboratory mice diverted researchers from this trail. Spencer's recent laboratory experiments with macaques reopens the case against *C. circinalis* (and its close relative, *C. revoluta)* by showing specific neurological damage following administration of the amino acid β-N-methylamino-L-alanine (L-BMAA). This toxin is concentrated in the seeds of certain Cycas species and is known to persist in traditionally processed cycad seed flour.

L-BMAA is closely related to β-N-oxalylamino-L-alanine (L-BOAA), isolated as the toxic ingredient of chickling pea *(Lathyrus sativus)* responsible for lathyrism, a similar neurological disorder. Both amino acids are excitotoxic and may cause convulsions in high doses. However, the toxic effects documented here are insidious in that they may not be apparent until 20 or 30 years following consumption. Such "slow toxins" may act by greatly accelerating the normal neuronal deterioration associated with aging. Their delayed action effects makes proving their etiological role very difficult.

Since 1955 the indicence of Guam ALS-PD has declined, apparently due to post-War dietary shifts leading to a greatly diminished reliance on cycad seed flour as a staple source of food energy. An association between traditional cycad consumption and degenerative neurological disease has also been suggested for certain Japanese, Indonesian, and Australian aboriginal populations. The low incidence of this syndrome on nearby Saipan may be due to the fact that German settlers there replaced indigenous cycad forests with sugar cane early in this century. The reliance for food on the seeds of the neotropical cycad *Dioone edule* in Mexico was not remarked upon. Your News and Comments editor would be pleased to receive any informed commentary on this issue for future discussion.

COMMISSION ON ETHNOBOTANY OF THE INTERNATIONAL UNION OF ANTHROPOLOGICAL AND ETHNOLOGICAL SCIENCES (IUAES)

The IUAES Executive Committee and Permanent Council approved the creation of the Commission on Ethnobotany in April of 1987. The Commission has been active informally since before 1985 under the direction of S. K. Jain (A-26 Mall Avenue Colony, *LUCKNOW*-226-001, U. T., INDIA), retired director of the Botanical Survey of

India and National Environmental Fellow of the Government of India. To date the Commission has collaborated with the Society of Ethnobotanists to publish *A World Directory of Ethnobotanists* listing over 500 names and is preparing for publication *A Manual of Ethnobotany* based on a training course held in March, 1986. The immediate goals of the Commission are:

1) to take stock of the present knowledge in the discipline,
2) to evaluate the potential of the subject,
3) to decide on priority areas for research,
4) to promote and support research on folk medicine, subsistence foods, and agriculture,
5) to make comparative studies and to evaluate existing baseline data,
6) to investigate the bases of folk classification and nomenclature and conservation practice,
7) to establish links among research centers and workers,
8) to report periodically to the IUAES on Commission activities,
9) to promote the interdisciplinary role of ethnobotany, and
10) to promote the wider objectives of the IUAES.

Commission membership stood at 19 in November, 1986, including Society of Ethnobiology stalwarts Janice Alcorn, Brent Berlin, Richard Ford, Eugene Hunn, and Nancy Turner. Other representatives from the U.S.A. and Canada are R. F. Chandler of Dalhousie University, T. Johns of McGill University, M. D. Merlin of the University of Hawaii at Manoa, G. T. Prance of the New York Botanic Garden, and R. E. Schultes of Harvard University. These commissioners would appreciate communications concerning your suggestions, activities, or plans relating to the goals listed above.

REVISED DATES FOR
SOCIETY OF ETHNOBIOLOGY
ELEVENTH ANNUAL CONFERENCE
MEXICO CITY, MEXICO

All events for the Eleventh Annual Conference in Mexico City, Mexico are now scheduled to occur one week earlier than previously announced. The pre-conference reception will be the evening of Wednesday, 9 March 1988, followed by two full days of oral presentations (Thursday and Friday, 10 and 11 March 1988) and one full day of poster sessions (Saturday, 12 March 1988). Field trips of one day duration and of three to four days duration are planned. A detailed announcement was published in Volume 7, Number 1. That information is still valid except for the dates.

A second mailing and Call for Papers is planned. If you haven't received a copy, you can obtain one by contacting either: Dr. Robert A. Bye, Jr., Jardin Botanico, U.N.A.M., Apdo. Post. 70-614, 04510 Mexico, DF, MEXICO, or Jan Timbrook, Department of Anthropology, Santa Barbara Museum of Natural History, 2559 Puesta del Sol Road, Santa Barbara, California 93105 USA.

ETHNOBIOLOGICAL HUMOR FROM *TUMBLEWEED'S WILD WEST*

Annie Gould of Etobicoke, Ontario, Canada, submitted these examples of "Poohawk Indian" ethnobotany for our enjoyment! The full Poohawk ethnography is available in T. K. Ryan's *Tumbleweed's Wild West*, Fawcett Gold Medal Books, New York (1986).

Ryan, T.K., 1986 *Tumbleweeds Wild West*, New York; Fawcett Gold Medal Book

Museu Paraense Emílio Goeldi
FIRST INTERNATIONAL CONGRESS OF ETHNOBIOLOGY
19-24 July, 1988, in Belém, Pará, Brazil

The First International Congress of Ethnobiology will be sponsored by the Núcleo de Etnobiologia of the MUSEU PARAENSE EMILIO GOELDI, in the Amazonian capital of Belém, Pará (Brazil), from 19 to 24 July, 1988. This preliminary notice is an invitation to all interested persons.

The six-day congress will be divided into three parts: 1. Three days devoted to specialized areas of ethnobiological research (e.g., ethnobotany, ethnoecology, ethnogeology-ethnopedology, ethnopharmacology-ethnomedicine, ethnozoology, ethnoentomology, ethnotaxonomy, etc.); 2. Two days devoted to interdisciplinary discussions arranged around geographic interests and area studies (e.g., humid tropics, savannas, mountains, steppes, etc. in South America, Central and North America, Eastern and Western Europe, Asia, India, Oceania, Northern and sub-Saharan Africa, etc.); 3. A final day is reserved presentations of applied ethnobiological projects and discussions about the potential of ethnobiological research for the improvement of world social and ecological conditions.

The organization of the congress is preliminary and suggestions are most kindly welcome. Participants in the congress may take part in as many programs as desired. Organized symposia are invited. Papers will be limited to 15 minute presentations, with 10 minutes reserved for discussion.

Special symposia will be announced in a later circular. Two key symposia are already planned: 1. "New Directions in Ethnobiology" and 2. "Applications of Ethnobiology in Conservation and Resource Management."

POST-CONGRESS: 24-29 JULY, 1988

A post-congress program will feature field excursions in the Amazon and discussions regarding ethnobiological research in Brazil and Latin America. Two special symposia are planned: 1. "Application of Folk and Indigenous Knowledge in the Conservation and Management of Amazônia" and 2. "Ethnomedicinal Research and its Application in Latin America."

For additional information, please write to: Prof. Dr. Darrell Addison Posey, Núcleo de Ethnobiologia, Museu Paraense Emílio Goeldi - CNPq, Caixa Postal 399, 66.000 Belém, Pará, BRAZIL. Telephone: (091) 228-2341.

PLEASE SEND NAMES AND ADDRESSES OF OTHERS
WHO MAY BE INTERESTED IN PARTICIPATING IN THE CONGRESS.

ETHNOBOTANIST ACHIEVES POP-HERO STATUS

Anne Fadiman's piece on "Dr. [Mark] Plotkin's Jungle Pharmacy: An Ethnobotanist Goes Native for Science" in the June, 1987 *Life* magazine (Volume 10, number 6, pp. 15-17) threatens to make "ethnobotanist" a household word! According to Fadiman, Plotkin's strategy for saving the Amazonian forests (he is director of plant conservation at the World Wildlife Fund in Washington, D.C.) is to "make the jungle profitable," most notably, by ferreting out the pharmacological secrets of the Tirio shamans (of southwest Suriname). Plotkin's research identified 239 species of plants used by the Tirio for "hallucinogens, aphrodisiacs and medicines." Plotkin remarked that, "The key [to convincing shamans to part with their esoteric knowledge] was convincing them that I was truly crazy about plants . . . Talking about leaf infusions with a shaman is like talking about the Redskins with a football fanatic."

The following financial report was provided by Secretary/Treasurer Steve Emslie:

SOCIETY OF ETHNOBIOLOGY
TREASURER'S REPORT 7 MARCH 1987
DATE OF LAST REPORT: 20 MARCH 1986
FUNDS IN ACCOUNT AT LAST REPORT: $3,694.46

INCOME SINCE LAST REPORT:

Sale of Backissues	$1,552.00
Membership Dues	10,276.80
Author's Reprints	830.00
Sale of T-shirts	453.50
Barbara Lawrence Fund	2,660.00
Interest	455.34
TOTAL INCOME THIS REPORT	$16,227.64
TOTAL INCOME + PREVIOUS ACCOUNT FUNDS	$19,922.10

EXPENSES SINCE LAST REPORT:

Major Expenses:

Journal Typesetting	$4,277.00
Journal Printing	3,962.72
Journal Postage	604.95
Various Minor Expenses	1,381.18
TOTAL EXPENSES SINCE LAST REPORT	$10,225.85
TOTAL INCOME IN ACCOUNT — TOTAL EXPENSES	$9,696.25
TOTAL FUNDS IN ACCOUNT AS OF 7 MARCH 1987	$7,587.36

NOTICE TO AUTHORS

The *Journal of Ethnobiology* accepts papers on original research in ethnotaxonomy and folk classification, ethnobotany, ethnozoology, cultural ecòlogy, plant domestication, zooarchaeology, archaeobotany, palynology, dendrochronology and ethnomedicine. Authors should follow the format for article organization and bibliographies from articles in this issue. All papers should be typed double-spaced with pica or elite type on 8½ x 11 inch paper with at least one inch margins on all sides. The ratio of tables and figures to text pages should not exceed 1:2-3. Tables should not duplicate material in either the text or graphs. All illustrations are considered figures and should be submitted reduced to a size which can be published within a journal page without further reduction. Photographs should be glossy prints of good contrast and sharpness with metric scales included when appropriate. All illustrations should have the author(s) name(s) written on the back with the figure number and a designation for the top of the figure. Legends for figures should be typed on a separate page at the end of the manuscript. Do not place footnotes at the bottom of the text pages; list these in order on a separate sheet at the end of the manuscript. Metric units should be used in all measurements. Type author(s) name(s) at the top left corner of each manuscript page; designate by handwritten notes in the left margin of manuscript pages where tables and graphs should appear.

If native language terminology is used as data, a consistent phonemic orthography should be employed, unless a practical alphabet or a more narrow phonetic transcription is justified. A brief characterization of this orthography and of the phonemix inventory of the language(s) described should be given in an initial note. To increase readability native terms should be indicated as ***bold-face italics*** to contrast with the normal use of *italic* type for foreign terms, such as latin binomials. If necessary, the distinction between lexical *glosses*, i.e., English language approximations of a term's referential meaning, and precise English equivalents or definitions should be indicated by enclosing the gloss in single quotation marks.

Authors must submit two copies of their manuscript plus the original copy and original figures. Papers not submitted in the correct format will be returned to the author. Submit your manuscripts to:

DR. WILLARD VAN ASDALL, Editor
Journal of Ethnobiology
Arizona State Museum, Building 26
University of Arizona
Tucson, Arizona 85721

NEWS AND COMMENTS

Individuals with information for the "News and Comments" section of the *Journal* should submit all appropriate material to Eugene Hunn, Department of Anthropology, DH-05, University of Washington, Seattle, Washington 98195.

BOOK REVIEWS

We welcome suggestions on books to review or actual reviews from the readership of the *Journal*. Please send suggestions, comments, or reviews to Terence E. Hays, Department of Anthropology and Geography, Rhode Island College, Providence, Rhode Island 02908.

SUBSCRIPTIONS

Subscriptions to the *Journal of Ethnobiology* should be addressed to Cecil H. Brown, Department of Anthropology, Northern Illinois University, Dekalb, Illinois 60115-2854. Subscription rates are $40.00, institutional; $20.00 regular members, for U.S., Canada, and Mexico; foreign subscribers add $8.00. Write checks payable to *Journal of Ethnobiology*. Defective copies or copies lost in shipment will be replaced if written request is received within one year of issue.

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